

Double Selection in Causal Inference*

Christian Alemán
New York University Abu Dhabi

Christopher Busch
LMU Munich
CESifo and CEPR

Raül Santaaulàlia-Llopis
New York University Abu Dhabi
BSE and CEPR

Tetyana Surovtseva
New York University Abu Dhabi

October 3, 2026

Abstract

We introduce a unified *double selection* framework accommodating group-specific additive and multiplicative shifters of an unobserved common time trend (CTT). Under *double selection*, the CTT does not manifest as a parallel trend; the resulting non-constant selection bias is a linear combination nesting standard difference-in-differences (DiD) in levels and logs. Opposing shifters can yield patterns structurally impossible under standard DiDs, such as a ranking reversal of the groups over time. We establish that in the presence of *double selection* bias treatment effects are non-parametrically identified by the change of the ratio of within-group outcome changes over time. The *double selection* structure induces a stable linear relationship in the outcome-on-outcome space, grounding a simple two-step estimator operating in levels. Revisiting [Card \(1992\)](#), *double selection* uncovers valid counterfactuals where standard DiD fails.

Keywords: Double Selection, Difference-in-Differences, Causal Inference, Nonparametric Identification

JEL Classification: C14, C21, J38

*We thank Richard Blundell, Xavier D'Haultfeuille, Fane Groes, Liyang Sun, Tomasz Olma, Luis Rojas, participants at seminars and conferences for helpful comments and suggestions. Alemán: christian.c.aleman@gmail.com; Busch: christopher.busch@econ.lmu.de; Santaaulàlia-Llopis: rs8561@nyu.edu; Surovtseva: ts4596@nyu.edu

"If there is a common trend in logs, there will not be one in levels and vice versa"
(Angrist and Pischke, 2009, p. 171).

1 Introduction

Difference-in-differences (DiD) is a workhorse method in empirical economics for identifying causal effects of policy interventions or natural experiments. At the core of this identification strategy lies the assumption that, in the absence of treatment, any systematic differences between the groups would have remained unchanged over time: in other words, DiD assumes a constant selection bias (CSB).¹ The validity of the constant selection bias assumption hinges critically on the scale in which outcomes are measured. When outcomes are expressed in levels, the DiD estimator assumes a constant *additive* difference between the treated and the control groups, whereas when expressed in logs, DiD assumes a constant *proportional* difference. Crucially, these two assumptions cannot hold simultaneously: if the selection bias is constant in levels, then it varies in logs, and vice versa; as noted by Angrist and Pischke (2009) in the leading quote. The same issue arises with any other monotonic transformation of the outcome variable.² Thus, the choice between levels and logs implicitly imposes assumptions about the data generating process (DGP) and can significantly impact causal inferences drawn (e.g. Meyer, 1995; Lechner et al., 2016; Kahn-Lang and Lang, 2020; McConnell, 2024).

Double Selection. In this context, we introduce a unified data generating process (DGP) featuring two types of individual-specific shifters: a level shifter that enters additively, and a proportional shifter that enters multiplicatively to scale an unobserved common time trend (CTT). As groups differ in both their average level and average proportional shifters—a phenomenon we term *double selection*—the selection bias between two groups is non-constant over time regardless of whether the outcome is expressed in levels or in logs. Thus, the CTT does not generally manifest as a parallel time trend. Further, we show that the bias emerging under double selection can be decomposed into a linear combination of the biases that arise under the two standard single-selection environments—namely, single selection in levels and single selection in logs. Thus, our proposed framework formally nests these two classical specifications as constrained subcases.

¹Combined with the Stable Unit Treatment Value Assumption (SUTVA) and the absence of Pre-Treatment Effects (NPE), a constant selection bias (CSB) delivers identification of the effect of treatment.

²The focus on the distinction between outcomes in levels versus logarithms is of central importance for empirical work. A survey of studies published in the *American Economic Review* and *Econometrica* over the past decade indicates that logarithmic transformations dominate roughly 60–70 percent of outcome specifications, whereas linear level specifications comprise approximately 20–30 percent. The remaining fraction typically employs alternative transformations, such as the inverse hyperbolic sine or quantile metrics.

Beyond its non-constant nature, we further characterize the behavior of this selection bias through its strict monotonicity in the unobserved common time trend, alongside its limiting values. Specifically, the group-level rankings on the proportional shifters amplify or attenuate the overall selection bias. In the limit, as the common time trend approaches zero, the level difference between the groups is pinned down exclusively by the difference between their additive level shifters; conversely, as the common time trend approaches infinity, the bias diverges at a rate dictated by the proportional shifters. This establishes a unique threshold of the common component which separates two regimes: one where the additive selection dominates and one where the proportional selection dominates. When these two shifters operate in opposing directions, the two regimes are also characterized by opposite signs and the threshold dictates a sign reversal where the expected untreated group outcomes intersect, generating a ranking reversal of the groups over time, a phenomenon strictly impossible under classical parallel-trends specifications.

Identification. Our main theoretical contribution is to establish a new non-parametric identification strategy for the average treatment effect on the treated (ATT). Together with the standard Stable Unit Treatment Value Assumption (SUTVA) and No Pre-Treatment Effects (NPE), identification is formalized under a *Double Constant Selection Bias* (Double CSB) condition. Double CSB does not impose a constant selection bias in levels or in logs, which would require parallel trends that are structurally unnecessary under double selection. Instead, identification is achieved by requiring a constant cross-group ratio of time changes in the untreated potential outcomes. Thus, all data moments needed to identify the treatment effect are low-dimensional group averages. The mechanic of this framework is that it separately pins down each latent selection component using distinct variation in pre-treatment observables: cross-group differences in outcome levels identify the additive selection, while the ratio of within-group changes over time separately identifies proportional selection. Consequently, the minimal data requirement to recover these parameters is two pre-treatment observations per group.

Estimation. While the selection bias is generally non-constant under double selection, we show that a constant difference in the two underlying shifters implies a stable, linear relationship between the expected outcomes of the two groups. This relationship becomes directly visible when recasting the model in an outcome-on-outcome representation, which holds regardless of whether the unobserved common time trend is deterministic or stochastic. Visually, on the 45-degree line, there is no selection bias. A parallel shift from the 45-degree line implies single selection via the additive shifter, whereas a shift in the slope anchored at the origin implies a purely multiplicative shifter. Finally, a concurrent shift in both the intercept and the slope characterizes the presence of double selection.

Guided by this structural insight, we develop a two-step estimation procedure operating in levels. Crucially, our approach yields causal estimates under minimal data requirements, requiring only a single additional pre-treatment period relative to the standard difference-in-differences design. In the first step, we use pre-treatment data to estimate a linear projection of group-level averages via a simple ordinary least squares regression, where the resulting coefficients capture additive (level) and multiplicative (proportional) selection. In the second step, we use these estimated coefficients to normalize the outcomes for individuals in the control group for all time periods. In the periods after treatment, this forms the basis of a counterfactual outcome for the treated group had the treatment not occurred—under the three assumptions SUTVA, NPE, and Double CSB.

To validate the framework, we propose a two-part testing procedure that evaluates the presence of double selection and constancy of the bias. First, we test whether the estimated coefficients significantly deviate from the theoretical values expected under no selection. Second, we implement standard pretesting procedures to verify whether the normalized control outcomes and the treated outcomes display parallel trends during the pre-policy horizon. Passing this diagnostic pretest implies that our normalization successfully eliminates the non-constant bias emerging from double selection. Finally, statistical inference is conducted via a non-parametric bootstrap that resamples the data across both steps, thereby capturing the full estimation uncertainty without relying on restrictive parametric assumptions.

Proof of Concept. The flexibility of our *double selection* framework resolves the functional form ambiguity that routinely confounds empirical designs in panel data. Rather than forcing applied researchers to make an ad hoc choice between levels or logs, our framework introduces a unified specification that encompasses both variants, thereby expanding the set of available control units. As an empirical proof of concept, we re-examine the classic environment of policy-induced wage changes surrounding the July 1988 California minimum wage increase ([Card, 1992](#)). In his original study, Card relies on a control group composed of workers in five states that did not experience a minimum wage hike, executing a difference-in-differences design on mean log wages to attribute a roughly five percent wage expansion to the policy intervention. Using the same data source over an extended horizon, our unrestricted two-step estimator uncovers selection bias in levels and weakly rejects additional proportional selection bias for the same control group. The statistically significant second-step event-study treatment effect estimate of approximately 5.0 percent in 1989 corroborates the Card estimate.

Beyond replicating this established benchmark, the primary power of the *double selection* framework lies in its capacity to uncover valid counterfactual conditions in scenarios that me-

chanically disqualify standard DiD specifications. We show this by considering alternative control groups for California comprised of workers in other federal states. For a set of states, we find an empirical constellation where average wages in California are both at a different level, and grow at a different rate when compared to these states: in the years preceding the minimum wage increase, the difference systematically widens or closes, with the latter case generating a *structural takeover*. Mapping these data into the outcome-on-outcome space reveals a stable linear relationship. The resulting normalization parameters successfully eliminate these compounding selection biases, delivering normalized control trajectories that display parallel pre-policy trends to California and yielding structurally credible policy impact estimates under double constant selection bias. This implies that one can evaluate the minimum wage policy even in the absence of the original Card control group. Specifically, we find eleven states which can serve as valid controls for California under the double selection framework, while single selection is rejected. Translated into percentages of the baseline average wage before policy, the significant ATTs under double selection for California range from 4.6 percent to 16.0 percent for 1989 across control states.

Literature. Our framework relates to the growing literature questioning the robustness of DiD estimates to functional form assumptions. Prior work has emphasized that the choice between levels and logs—while often made ad hoc—has direct implications for the validity of the parallel trends assumption (Finkelstein, 2007; Powell and Leider, 2020; Park and Powell, 2021). Indeed, in his textbook, Cunningham (2021) observes: *"Very little thought if any is given to how exactly we should model some outcome. Just to take one example, should we use the log or the levels themselves? Should we use the quartic root? Should we use rates? These, it turns out, are critically important because for many of them, the parallel trends assumption needed for identification will not be achieved—even though it will be achieved under some other unknown transformation. (...) You must make strong commitments about the functional form itself."*

Our contribution differs from approaches that build around transformations of the outcome variable. For example, Athey and Imbens (2006) show that identification in DiD models can be achieved without relying on the linear-additive structure of outcomes by exploiting invariance to monotonic transformations of unobserved heterogeneity. Their Changes-in-Changes framework relaxes functional form assumptions by focusing on the distributional evolution of untreated outcomes within groups, allowing for nonlinearity and heterogeneity in treatment effects. Building on related ideas, recently, Roth and Sant'Anna (2023) identify strict conditions, such as random assignment or stationarity in the distribution of untreated potential outcomes, under which the parallel trends assumption holds regardless of the functional form for strictly monotonic transformations of the outcome. They do this within the framework of the canonical DiD model, and as such in a context with a single type of selection: the objective of their approach is to determine

the units in which a common time trend manifests as a parallel trend. In contrast, we explore a data-generating process with two types of selection bias, additive and proportional, and work out identification without requiring parallel time trends. We also avoid common issues associated with log transformations, such as undefined values at zero, by estimating directly in levels and using an outcome-on-outcome normalization ([Meyer, 1995](#); [Chen and Roth, 2024](#)).

The framework introduced here is part of a growing literature that explores data-generating processes featuring interactive fixed effects, a literature largely initiated by [Bai \(2009\)](#). While we exploit this underlying structure—where individual multiplicative shifters interact with an unobserved common time trend—we depart sharply from the statistical focus of traditional factor models. Conventional factor approaches typically treat individual loadings as high-dimensional parameters that must be consistently estimated, regularized, or explicitly projected out using large- N , large- T asymptotics. We establish that the full factor identification is an unnecessary detour under the conditions satisfied by our DGP. We bypass the latent parameter space entirely by proving identification nonparametrically using low-dimensional pre-policy group means. Rather than treating these shifters as high-dimensional nuisance parameters, we leverage them strictly as a structural foundation to resolve functional form ambiguity.

Thus, our approach contrasts the dominant strand of the literature on identifying policy effects in the presence of interactive fixed effects (e.g., [Gobillon and Magnac, 2016](#); [Brown and Butts, 2025](#)) that typically builds its identification strategy around first estimating the latent factor structure using pre-policy data to strip it out from the remaining variation. A complementary approach by [Callaway and Karami \(2023\)](#) recognizes that the factor model itself does not need to be fully identified, focusing instead on the relationship it induces between groups by using an observed proxy. We share Callaway and Karami’s core insight that full factor identification is unnecessary when evaluating a binary treatment effect. Our departure lies in how we recover this group relationship. Specifically, the core of our approach is that the relationship between group-level outcomes induced by the double selection model can be directly identified from pre-policy outcomes at the group-level. The stable relationship between groups, around which we built our estimator, also lies at the heart of synthetic control approaches ([Abadie and Gardeazabal, 2003](#); [Abadie et al., 2010](#)) as emphasized by [Athey et al. \(2018\)](#). Indeed, the outcome-on-outcome regression of our first step resembles synthetic control regressions extended by a constant as in the synthetic DiD estimator by [Arkhangelsky et al. \(2021\)](#). Against this background, an additional contribution of our paper is that it develops the interpretation of the interactive fixed effects model as establishing a bridge between DiD specified in levels (level selection) and DiD specified in logs (proportional selection), while providing a foundation for linear outcome-on-outcome relationships between groups that is central to synthetic control approaches.

The paper is organized as follows. In Section 2, we describe the empirical setting, introduce the concept of *double selection*, characterize its selection bias over time, and establish a linear relationship between expected outcomes across groups. In Section 3, we prove identification of treatment effects in the context of *double selection*. In Section 4, we introduce an estimator and discuss pre-trend testing. In Section 5, we illustrate our framework in an application. In Section 6, we briefly discuss extensions and finally, in Section 7 we conclude.

2 A DGP with Double Selection

Standard DiD models restrict selection bias between treatment and control groups to be either additive (DiD in levels) or proportional (DiD in logs), but not both. In this section, we discuss how a data-generating process (DGP) which features interactive fixed effects accommodates the simultaneous presence of both forms of selection; a setting which we refer to as *double selection*. This framework nests single selection as special instances and provides the foundation for the identification and estimation strategy developed in the following sections. We characterize the conditions under which constant single-selection cases emerge and highlight the implicit restrictions they impose. When neither structure fully captures the data, the resulting selection bias is a time-varying linear combination of both types.

2.1 The Data-Generating Process

Let q_{it} denote the level of some outcome of interest for individual i at time t . In order to allow for a log representation, we restrict $q_{it} \in \mathbb{R}^{++}$ here, but remove this restriction later. There is a common aggregate outcome component, $\lambda_t \in \mathbb{R} \forall i, t$ with $|\lambda_t| < \infty$. This common component shifts outcomes for all individuals in the absence of policy treatment, and it can be deterministic or stochastic. At some time t_p a policy is introduced in some groups, which affects the outcome starting in that period in a flexible way.

The described DGP can be summarized as

$$q_{it} = \underbrace{\theta_{0i} + \theta_{1i}\lambda_t + \varepsilon_{it}}_{=y_{it}} + \mathbb{1}_{t \geq t_p} \gamma_{it}, \quad (1)$$

where $\theta_{0i} \in \mathbb{R}$ is a level shifter and $\theta_{1i} \in \mathbb{R}^{++}$ is a proportional shifter. Both shifters are constant over time. ε_{it} is an idiosyncratic shock with $\mathbb{E}_i[\varepsilon_{it}] = 0$ and constant variance $\text{Var}[\varepsilon_{it}] = \mathbb{E}_i[\varepsilon_{it}^2] - \mathbb{E}_i[\varepsilon_{it}]^2 = \sigma^2$ with $0 < \sigma^2 < \infty$, where operator $\mathbb{E}_i[\cdot]$ denotes the expectation across individuals i at some t . Finally, $\mathbb{1}_{t \geq t_p}$ is an indicator function equal to one if $t \geq t_p$, and zero otherwise, and γ_{it} captures the policy effect for periods $t \geq t_p$ for an individual that is subject

to a policy change treatment in period t_p ; $\gamma_{it} = 0$ for an untreated individual. We denote the outcome in the absence of policy by y_{it} .

2.2 Characterization of Selection Bias Over Time

We next discuss several properties of this DGP, explain how differences between group-averages of parameters θ_{0i} and θ_{1i} shape the differences between groups—and in particular, how these differences translate into generally time-varying selection bias in levels and logs. To lay the groundwork for our analysis of selection bias, we first formalize the relationship between the level and log specifications of the DGP. Lemma 1 shows how the DGP can be equivalently expressed in multiplicative form under mild conditions, allowing us to characterize group-level differences on both scales and to define bias in levels and logs in a unified framework.

Lemma 1. *Consider the DGP in (1) absent policy. If $y_{it} = \theta_{0i} + \theta_{1i}\lambda_t + \varepsilon_{it} \in \mathbb{R}^{++}$ and $\mathbb{E}[\varepsilon_{it}|\theta_{0i}, \theta_{1i}, \lambda_t] = 0$, then:*

(a) *The individual outcome path absent policy can be equivalently formulated as:*

$$y_{it} = (\theta_{0i} + \theta_{1i}\lambda_t) u_{it}, \quad \text{with } \mathbb{E}_i[u_{it}|i \in g] = 1 \quad (2)$$

(b) *For small values of $u_{it} - 1$, the conditional expectation of $\ln(u_{it})$ can be approximated as:*

$$\mathbb{E}_i[\ln(u_{it})|i \in g] \approx \mathbb{E}_i[u_{it} - 1|i \in g]$$

Proof: See Appendix A.1.

Equipped with this connection between an additive error term ε_{it} and a proportional error term u_{it} , Definition 1 formalizes the selection bias across groups as the difference between the expected outcome of group g and the expected outcome of a reference group g^* , expressed in levels or logs, and evaluated in the absence of policy. For the purpose of this illustration we consider a setting with small error terms such that the approximation in Lemma 1(b) holds.

Definition 1. The selection bias induced by the DGP in (1), either in levels or logs, is:

$$\begin{aligned} \text{Levels: } & \mathbb{E}_i[y_{it}|i \in g] - \mathbb{E}_i[y_{it}|i \in g^*] \\ &= (\mathbb{E}[\theta_{0i}|i \in g] + \mathbb{E}[\theta_{1i}|i \in g]\lambda_t) - (\mathbb{E}[\theta_{0i}|i \in g^*] + \mathbb{E}[\theta_{1i}|i \in g^*]\lambda_t) \quad (\text{not constant}) \end{aligned} \quad (3)$$

$$\begin{aligned} \text{Logs: } & \mathbb{E}_i[\ln y_{it}|i \in g] - \mathbb{E}_i[\ln y_{it}|i \in g^*] \\ &= \mathbb{E}_i[\ln((\theta_{0i} + \theta_{1i}\lambda_t)u_{it})|i \in g] - \mathbb{E}_i[\ln((\theta_{0i} + \theta_{1i}\lambda_t)u_{it})|i \in g^*] \\ &= \mathbb{E}_i[\ln(\theta_{0i} + \theta_{1i}\lambda_t)|i \in g] - \mathbb{E}_i[\ln(\theta_{0i} + \theta_{1i}\lambda_t)|i \in g^*] \\ &\quad + \mathbb{E}_i[\ln u_{it}|i \in g] - \mathbb{E}_i[\ln u_{it}|i \in g^*] \\ &\approx \mathbb{E}_i[\ln(\theta_{0i} + \theta_{1i}\lambda_t)|i \in g] - \mathbb{E}_i[\ln(\theta_{0i} + \theta_{1i}\lambda_t)|i \in g^*] \quad (\text{not constant}) \end{aligned} \quad (4)$$

where the last line uses the approximation $\mathbb{E}_i[\ln(u_{it})|i \in g] \approx \mathbb{E}_i[u_{it} - 1|i \in g]$ (from Lemma 1(b)), and the value of the expectation $\mathbb{E}_i[u_{it}|i \in g] = 1$ (from Lemma 1(a)).

Selection Bias Over Time. The selection terms in Definition 1 directly show that selection bias between groups is generally not constant (neither in levels nor in logs) in the presence of both additive and proportional shifters, as long as the common component λ_t varies over time, which we also summarize in Lemma A.1 in Appendix A. In particular, non-constancy of the selection bias in levels is induced by group differences in the proportional shifters, and non-constancy of the selection bias in logs is induced by the presence of non-zero level shifters. Remember that the constancy of selection bias lies at the core of DiD identification. Thus, in order to build intuition for the implications of this non-constancy, we now discuss how the selection terms evolve over time.

To structure this discussion, we consider four different parameter constellations of the DGP in (1). Formally, we introduce constraints on vectors $\theta_0 = (\theta_{01}, \dots, \theta_{0n}) \in \Theta_0 = \mathbb{R}^n$ and $\theta_1 = (\theta_{11}, \dots, \theta_{1n}) \in \Theta_1 = \mathbb{R}^{n,++}$. We consider two constraints on the level and proportional shifters: (C0) imposes a zero level shifter for individuals i , and (C1) restricts all individuals to share the same proportional shifter. Depending on whether constraint (C0), (C1), both, or neither hold, the DGP in Equation (1) gives rise to four distinct patterns of selection bias. In two of those cases there is *single constant selection bias* (single CSB), either in levels or in logs—depending on which of the two constraints holds. In one case there is a *double constant selection bias*—arising when none of the constraints hold and both the level and proportional shifters differ between groups. Finally, there is the case of no selection bias—when both constraints hold. Table 1 summarizes these subcases and refers to Propositions 1 and 2, and Corollary 1, which formally state the implications of the different parameter restrictions for the arising selection bias.

Table 1: Selection Bias in DGP (1), Subcases

$\theta_0 \backslash \theta_1$	$\theta_1 \in \Theta_1 \mathbf{C1}$	$\theta_1 \notin \Theta_1 \mathbf{C1}$
$\theta_0 \in \Theta_0 \mathbf{C0}$	No Selection Bias Corollary 1	Single Constant Selection Bias, Logs Proposition 1(b)
$\theta_0 \notin \Theta_0 \mathbf{C0}$	Single Constant Selection Bias, Levels Proposition 1(a)	Double Constant Selection Bias Proposition 2

Notes: Selection bias arising from DGP (1) where the set $\Theta_0 | \mathbf{C0}$ collects the vector θ_0 for which constraint (C0) holds, and $\Theta_1 | \mathbf{C1}$ collect the vectors θ_1 for which constraint (C1) holds.

Proposition 1. Single Constant Selection Bias: Let (C0) and (C1) denote constraints on the group-specific level shifters and proportional shifters, respectively:

Constraint 0 (C0): $\theta_{0i} = 0 \forall i$

Constraint 1 (C1): $\theta_{1i} = \bar{\theta}_1 \forall i$

Let $\Theta_0 | \mathbf{C0}$ collect all vectors θ_0 for which constraint (C0) holds, and $\Theta_1 | \mathbf{C1}$ collect the vectors θ_1 for which constraint (C1) holds.

(a) Levels: If $\theta_0 \notin \Theta_0 | \mathbf{C0}$ and $\theta_1 \in \Theta_1 | \mathbf{C1}$, then the DGP in (1) exhibits a constant selection bias (CSB) in levels, and not in logs.

(b) Logs: If $\theta_0 \in \Theta_0 | \mathbf{C0}$ and $\theta_1 \notin \Theta_1 | \mathbf{C1}$, then the DGP in (1) exhibits a constant selection bias (CSB) in logs, and not in levels.

Proof: see Appendix A.2.

When both the additive and proportional components are identical across groups, as specified in Corollary 1, the expected outcomes coincide at all points in time, and there is no selection bias. This corresponds to treated and control groups tracing out the same path in the absence of treatment. While this case rarely holds in practice, it provides a natural benchmark for comparing the single- and double-selection scenarios.

Corollary 1. If $\theta_0 \in \Theta_0 | \mathbf{C0}$ and $\theta_1 \in \Theta_1 | \mathbf{C1}$, then there is neither selection bias in levels nor in logs.

The taxonomy defined by Proposition 1 and Corollary 1 delineates the special cases in which selection bias is constant in either levels or logs, or absent altogether. When neither constraint is imposed, group differences at any scale reflect both additive and proportional components. In this more general case, which we refer to as double selection, the selection bias terms in levels and logs become time-varying linear combinations of the two single-selection components. Proposition 2 formalizes this result.

Proposition 2. Double Constant Selection Bias: *If $\theta_0 \notin \Theta_0|C0$ and $\theta_1 \notin \Theta_1|C1$, then*

- (a) *The selection bias in levels in (3) is a linear combination of the selection bias that arises under constraint (C0) and the selection bias that arises under constraint (C1). The weights on the two terms vary with the common component λ_t .*
- (b) *The selection bias in logs in (4) can be approximated by a linear combination of the selection bias that arises under constraint (C0) and the selection bias that arises under constraint (C1). The weights on the two terms vary with the common component λ_t . In the case of degenerate distributions of the scaling parameters, i.e., $\theta_{1i} = \bar{\theta}_1 \forall i \in g^*$ and $\theta_{0i} = \bar{\theta}_0 \forall i \in g^*$, this approximation is exact.*

Proof: see Appendix A.3.

Overall, the size of the group differences in the level shifters and the proportional shifters, as well as the magnitude of λ_t , determine whether selection in levels or selection in logs dominates. The following Lemma 2 establishes strict monotonicity in λ_t of the selection bias in levels and logs. Depending on whether the mean proportional shifter is greater or smaller in group g relative to group g^* , the level difference between the groups increases or decreases as λ_t increases.

Lemma 2. Strict Monotonicity of the Selection Bias (SB) in λ : *The selection bias (SB) in levels is given by $\mathbb{E}_i[y_{it}|i \in g] - \mathbb{E}_i[y_{it}|i \in g^*]$. The SB in logs is $\mathbb{E}_i[\ln y_{it}|i \in g] - \mathbb{E}_i[\ln y_{it}|i \in g^*]$. Under DGP (1), the SB in levels and logs are monotone in λ :*

- (a) *The SB in levels is strictly monotonically increasing (decreasing) in λ iff $\mathbb{E}_i[\theta_{1i}|i \in g] > \mathbb{E}_i[\theta_{1i}|i \in g^*]$ ($\mathbb{E}_i[\theta_{1i}|i \in g] < \mathbb{E}_i[\theta_{1i}|i \in g^*]$).*
- (b) *The SB in logs is strictly monotonically increasing in λ if $\left(\frac{\theta_{0i}}{\theta_{1i}}\right)|g \prec_{FOSD} \left(\frac{\theta_{0i}}{\theta_{1i}}\right)|g^*$, i.e., $\left(\frac{\theta_{0i}}{\theta_{1i}}\right)|g^*$ first-order stochastically dominates $\left(\frac{\theta_{0i}}{\theta_{1i}}\right)|g$. Likewise, SB is monotonically decreasing in λ if $\left(\frac{\theta_{0i}}{\theta_{1i}}\right)|g \succ_{FOSD} \left(\frac{\theta_{0i}}{\theta_{1i}}\right)|g^*$.*

Proof. See Appendix A.4.1

□

In the limit, the level difference is pinned down by the difference between the level shifters as λ_t goes to zero, and goes to (positive or negative) infinity as λ_t goes to infinity. We show this property in the following lemma:

Lemma 3. Limiting Values of the Selection Bias: *The selection bias (SB) in levels is given by $\mathbb{E}_i[y_{it}|i \in g] - \mathbb{E}_i[y_{it}|i \in g^*]$. The SB in logs is $\mathbb{E}_i[\ln y_{it}|i \in g] - \mathbb{E}_i[\ln y_{it}|i \in g^*]$. Under DGP (1), the limits of SB in levels and logs in as λ approaches either zero or infinity are as follows.*

(a) Denote the SB in levels by $\Delta_{levels} = \mathbb{E}_i[y_{it}|i \in g] - \mathbb{E}_i[y_{it}|i \in g^*]$. The limits of the SB in levels are:

$$\lim_{\lambda \rightarrow 0} \Delta_{levels} = \mathbb{E}_i[\theta_{0i}|i \in g] - \mathbb{E}_i[\theta_{0i}|i \in g^*] \quad (5)$$

$$\lim_{\lambda \rightarrow \infty} \Delta_{levels} = \begin{cases} \infty, & \text{if } \mathbb{E}_i[\theta_{1i}|i \in g] > \mathbb{E}_i[\theta_{1i}|i \in g^*] \\ -\infty, & \text{if } \mathbb{E}_i[\theta_{1i}|i \in g] < \mathbb{E}_i[\theta_{1i}|i \in g^*] \end{cases} \quad (6)$$

(b) Denote the SB in logs by $\Delta_{logs} = \mathbb{E}_i[\ln y_{it}|i \in g] - \mathbb{E}_i[\ln y_{it}|i \in g^*] = \mathbb{E}_i[\ln(\theta_{0i} + \theta_{1i}\lambda)|i \in g] - \mathbb{E}_i[\ln(\theta_{0i} + \theta_{1i}\lambda)|i \in g^*]$. For $\theta_{0i} > 0$ a.s. and $\theta_{1i} > 0$ a.s., the limits of the SB in logs are:

$$\lim_{\lambda \rightarrow 0} \Delta_{logs} = \mathbb{E}_i[\ln(\theta_{0i})|i \in g] - \mathbb{E}_i[\ln(\theta_{0i})|i \in g^*] \quad (7)$$

$$\lim_{\lambda \rightarrow \infty} \Delta_{logs} = \mathbb{E}_i[\ln(\theta_{1i})|i \in g] - \mathbb{E}_i[\ln(\theta_{1i})|i \in g^*]. \quad (8)$$

Proof. See Appendix A.4.2 □

Similarly, the log difference increases or decreases with λ_t depending on the relative distributions of the level and proportional shifters between the two groups. In the limit, the log difference is pinned down by the (log) difference between the level shifters as λ_t goes to zero, and by the (log) difference between the proportional shifters as λ_t goes to infinity. This leads to the following proposition.

Proposition 3. Size-Dominance Selection: *Consider DGP (1) in the presence of double selection, i.e., $\theta_0 \notin \Theta_0|\mathbf{C0}$ and $\theta_1 \notin \Theta_1|\mathbf{C1}$.*

(i) **SB in Levels.** *There is a unique threshold $\bar{\lambda}$, below which the SB in levels is dominated by the level shifter differences across groups as opposed to the proportional shifter differences*

across groups:

$$\left| \underbrace{\mathbb{E}_i[\theta_{0i}|i \in g] - \mathbb{E}_i[\theta_{0i}|i \in g^*]}_{= \text{CSB in Levels}} \right| > \left| \underbrace{(\mathbb{E}_i[\theta_{1i}|i \in g] - \mathbb{E}_i[\theta_{1i}|i \in g^*]) \lambda_t}_{= \text{Level counterpart of CSB in Logs}} \right| \Leftrightarrow |\lambda_t| < |\bar{\lambda}| \quad (9)$$

and the opposite otherwise.

(ii) SB in Logs. If the distribution of level and proportional shifters is degenerate within each group, i.e., $\theta_{0i} = \bar{\theta}_{0g} \forall i \in g$ and $\theta_{1i} = \bar{\theta}_{1g} \forall i \in g$, and likewise for g^* , and $\lambda_t \in \mathbb{R}^{++}$ then, below the threshold obtained in (9), the SB in logs is dominated by the level shifter differences across groups as opposed to the proportional shifter differences across groups:

$$\lambda_t \leq \bar{\lambda} \Rightarrow \left| \underbrace{\ln(\bar{\theta}_{0g} + \bar{\theta}_1 \lambda_t) - \ln(\bar{\theta}_{0g^*} + \bar{\theta}_1 \lambda_t)}_{= \text{Log counterpart of CSB in Levels}} \right| \geq \left| w(\theta_0, \theta_1, \lambda_t) \underbrace{(\ln \bar{\theta}_{1g} - \ln \bar{\theta}_{1g^*})}_{= \text{CSB in Logs}} \right|, \quad (10)$$

where $w(\theta_0, \theta_1, \lambda_t)$ is a weight that given level and proportional shifters (θ_0 and θ_1) changes with λ_t .

Proof: see Appendix A.5.

A key property of the double selection framework is that the trajectories of the treated and control groups can exhibit takeovers. Specifically, if the two distinct forms of selection bias operate in opposing directions—where one group is favored by the additive level shifter while the other dominates the proportional shifter—the relative rankings of the group outcomes can invert over time. This selection bias reversal is impossible under standard DID frameworks, where the common time trend is forced to be parallel. This inversion occurs exactly at the size-dominance threshold derived in Proposition 3. The following corollary establishes this result.

Corollary 2. Sign-Dominance Selection (in Levels): The sign-dominance behavior of the selection bias in levels, $\mathbb{E}_i[y_{it}|i \in g] - \mathbb{E}_i[y_{it}|i \in g^*]$, is characterized as follows:

1. **Uniform Dominance (Same-Sign Shifters):** If $\text{sign}(\bar{\theta}_{0g} - \bar{\theta}_{0g^*}) = \text{sign}(\bar{\theta}_{1g} - \bar{\theta}_{1g^*})$, then the selection bias maintains a constant sign across all periods $\forall \lambda_t > 0$:

- **(1.i) Dominance of group g :** If $\bar{\theta}_{0g} > \bar{\theta}_{0g^*}$ and $\bar{\theta}_{1g} > \bar{\theta}_{1g^*}$, then $\mathbb{E}_i[y_{it}|i \in g] > \mathbb{E}_i[y_{it}|i \in g^*]$ for all λ_t . Thus, group g always dominates group g^* .

- **(1.ii) Dominance of group g^* :** If $\bar{\theta}_{0g^*} > \bar{\theta}_{0g}$ and $\bar{\theta}_{1g^*} \geq \bar{\theta}_{1g}$, then $\mathbb{E}_i[y_{it}|i \in g^*] > \mathbb{E}_i[y_{it}|i \in g]$ for all λ_t . Thus, group g^* always dominates group g .

2. **Dominance Reversal (Opposite-Sign Shifters):** If $\text{sign}(\bar{\theta}_{0g} - \bar{\theta}_{0g^*}) = -\text{sign}(\bar{\theta}_{1g} - \bar{\theta}_{1g^*})$, there exists a unique, strictly positive common time trend threshold $\bar{\lambda}_s = \frac{|\bar{\theta}_{0g} - \bar{\theta}_{0g^*}|}{|\bar{\theta}_{1g} - \bar{\theta}_{1g^*}|} > 0$ at which a sign reversal occurs:

- For $\lambda_t < \bar{\lambda}_s$, the sign of the selection bias in levels is determined entirely by the level shifter difference $(\bar{\theta}_{0g} - \bar{\theta}_{0g^*})$.
- For $\lambda_t = \bar{\lambda}_s$, the expected outcomes exactly cross, resulting in zero selection bias.
- For $\lambda_t > \bar{\lambda}_s$, the sign of the selection bias in levels flips and is completely dominated by the proportional shifter difference $(\bar{\theta}_{1g} - \bar{\theta}_{1g^*})$.

This threshold $\bar{\lambda}_s$ is identical to $\bar{\lambda}$ obtained in Proposition 3.

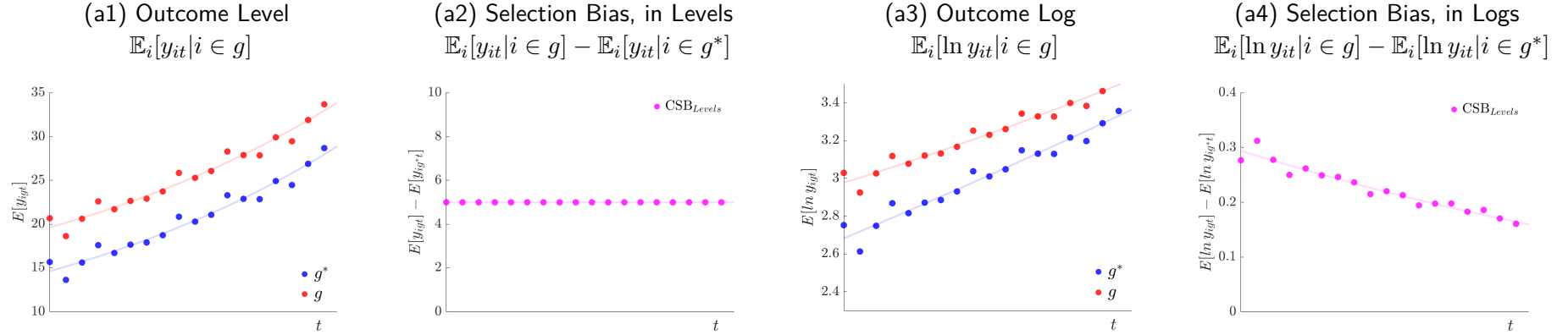
Proof. See Appendix A.6 □

Illustration of Selection Bias. Figure 1 illustrates the two cases of single selection bias between two groups characterized in Proposition 1 and highlights that constant selection bias in one scale typically implies non-constant bias in the other. The paths are based on restricted versions of the data-generating process (1), where one implies constant selection bias in levels (Panel (a)), and one implies constant selection bias in logs (Panel (b)). For this illustration, we consider degenerate distributions of the individual specific shifters within each group, i.e., $\theta_{0i} = \bar{\theta}_{0g} \forall i \in g$ and $\theta_{1i} = \bar{\theta}_{1g} \forall i \in g$, and likewise for g^* .

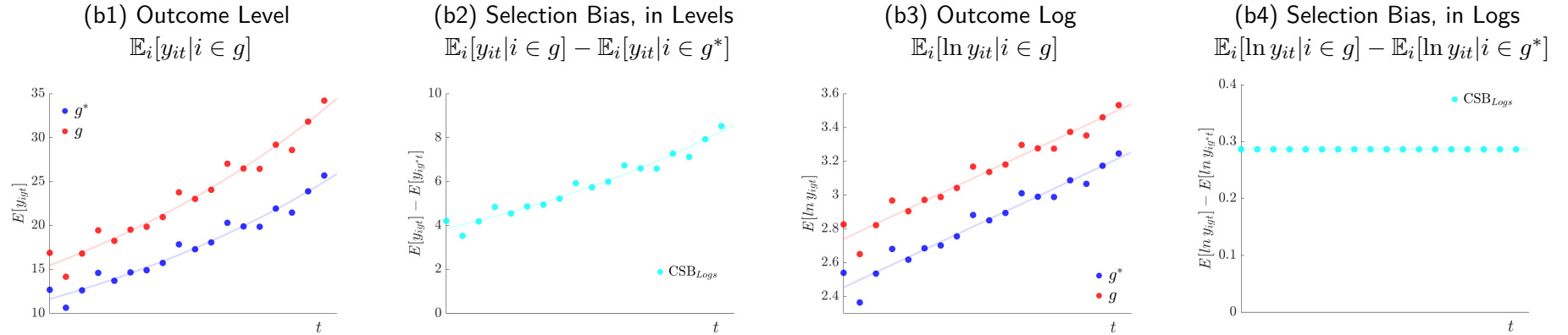
In both panels, subpanels (1) and (2) display group-level outcomes and selection bias in levels, while subpanels (3) and (4) show outcomes and selection bias in logs. In Panel (a), the group-specific differences are governed by additive shifts $\bar{\theta}_{0g}$, with the same proportional component across groups ($\bar{\theta}_{1g} = \bar{\theta}_{1g^*}$). As a result, selection bias is constant in levels: the treated and control groups exhibit parallel trajectories in subpanel (a1), and the difference in outcomes is constant in subpanel (a2). However, for a given level difference, the proportional gap depends on the scale of the common component λ_t such that the log-difference varies over time, as shown in subpanels (a3) and (a4). In contrast, Panel (b) features a purely proportional shift (induced by $\bar{\theta}_{0g} = \bar{\theta}_{0g^*} = 0$, $\bar{\theta}_{1g} \neq \bar{\theta}_{1g^*}$), which results in time-varying selection bias in levels as seen in subpanels (b1) and (b2), and a constant selection bias in logs as seen in subpanels (b3) and (b4).

Figure 1: Single Selection

(a) Single CSB in Levels: Constraint (C1)



(b) Single CSB in Logs: Constraint (C2)



Notes: Lines plot the deterministic component of the DGP; scattered dots plot the DGP including the stochastic component. Panel (a) shows a case with single selection in levels, panel (b) shows a case with single selection in logs. On the figure axes, $E[y_{igt}]$ is short-hand notation for $\mathbb{E}_i[y_{it}|i \in g]$, and accordingly for group g^* and for the log outcomes.

We next turn to an illustration of double selection. To this end, Figures 2 and 3 illustrate the essence of Proposition 2: when both additive and proportional components are different between groups, selection bias is generally not constant in either levels or logs. Figure 2 shows an example with a deterministic common component λ_t which is growing over time, where group g has a greater level shifter than group g^* , $\bar{\theta}_{0g} > \bar{\theta}_{0g^*}$, but a smaller proportional shifter, $\bar{\theta}_{1g} < \bar{\theta}_{1g^*}$. This places the setting within the realm of the regime takeover established in Proposition 2, displaying a time-varying and non-monotonic selection bias in both levels (panels (a) and (b)) and logs (panels (c) and (d)).

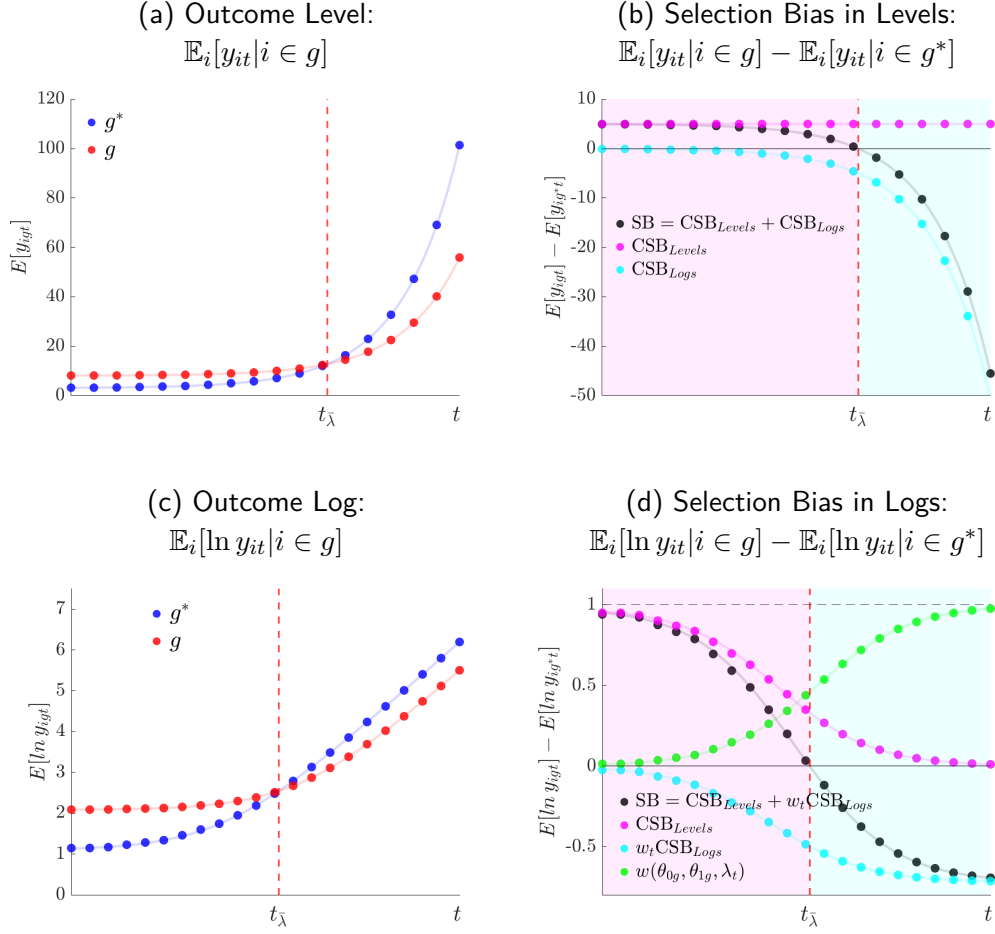
Here, the growing common component λ_t drives a complete sign reversal over time. Initially, for $\lambda_t < \lambda_s$, the group outcomes are dominated by the level shifters, meaning $\mathbb{E}_i[y_{it}|i \in g] > \mathbb{E}_i[y_{it}|i \in g^*]$. As λ_t grows past the size-dominance threshold $\bar{\lambda}$ (Proposition 3), the proportional shifters begin to dictate the scale of the bias. Eventually, at the crossing period $t = t_{\bar{\lambda}_s}$ (where $\lambda_t = \bar{\lambda}_s$), the expected outcomes exactly intersect. In subsequent periods where $\lambda_t > \bar{\lambda}_s$, the proportional shifter difference entirely overrides the level difference, reversing the group ranking such that $\mathbb{E}_i[y_{it}|i \in g] < \mathbb{E}_i[y_{it}|i \in g^*]$, in line with Proposition 2.

Panel (b) shows the overall selection bias in black, and its two components: The constant selection bias term in levels (CSB_{levels}) in pink, and the time-varying selection bias term which reflects the constant proportional difference (CSB_{Logs}) in cyan. The background color scheme marks the time periods in which the level and the proportional selection bias terms dominate the magnitude of the overall selection bias. In panel (d), we plot the corresponding elements of the proportional selection bias. In addition, the green line in this panel shows the weight given to CSB_{Logs} , which increases with t , as the common component increases.

Figure 3 further illustrates that these patterns persist under alternative specifications of λ_t : one deterministic, where both types of selection go in the same direction, and one stochastic. Selection bias varies systematically with the common component—and whether the term reflecting level differences ($\bar{\theta}_{0g} \neq \bar{\theta}_{0g^*}$) or the term reflecting proportional differences ($\bar{\theta}_{1g} \neq \bar{\theta}_{1g^*}$) is larger depends on the size of the common component, λ_t .³ This induces the potential for non-monotonic behavior of the selection bias over time, and for sign reversals in selection bias, even in the absence of policy treatment as is the case in Figure 2.

³In the appendix to this section, Figure A.1 plots output and selection bias in levels against the common component λ_t directly, which highlights that the structure of selection bias under double selection depends on the magnitude of the aggregate component, not on its time-series properties. At low values of λ_t , bias is primarily driven by level differences, while at high values, proportional differences dominate. The transition between these regimes occurs smoothly around a threshold $\bar{\lambda}$.

Figure 2: Double Constant Selection Bias (Case of Opposite-Sign Shifters)



Notes: The figure illustrates double selection in opposite directions, with $\bar{\theta}_{0g} > \bar{\theta}_{0g^*}$ and $\bar{\theta}_{1g^*} > \bar{\theta}_{1g} > 0$. On the figure axes, $E[y_{igt}]$ is short-hand notation for $\mathbb{E}_i[y_{it}|i \in g]$, and accordingly for group g^* and for the log outcomes.

2.3 Outcome-on-Outcome Representation

Although the relative importance of level and log selection varies with the common component, and thus varies over time, the structure of the DGP imposes a stable relationship between any two groups: The expected outcome of one group is a linear transformation of the outcome of another. This insight motivates a shift in perspective: rather than comparing the outcomes of treated and control groups in levels or logs in the cross-section, we examine the comovement of group-level outcomes. Proposition 4 formalizes the result and shows that the slope and intercept of a linear mapping reflect the differences between groups in the level and proportional shifters.

Figure 3: Double CSB: Illustrating Alternative Common Time Trends λ_t

(a) A Deterministic CTT:

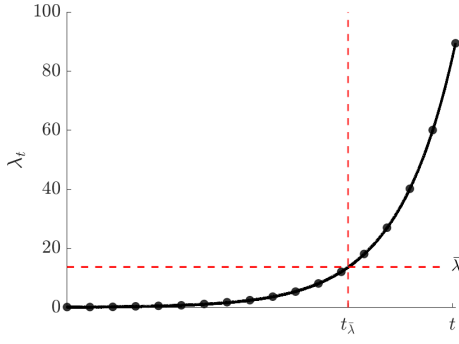
$$\lambda_t = \lambda_0(1 + l_\lambda)^t$$

(b) A Stochastic CTT:

$$\lambda_t = \bar{\lambda}e^{\eta_t} \text{ and } \eta_t \sim iid(0, \sigma_\eta^2)$$

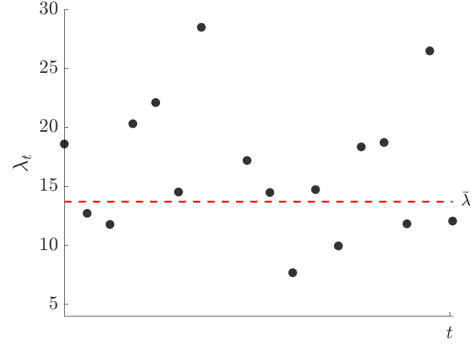
(a1) CTT:

$$\lambda_t$$



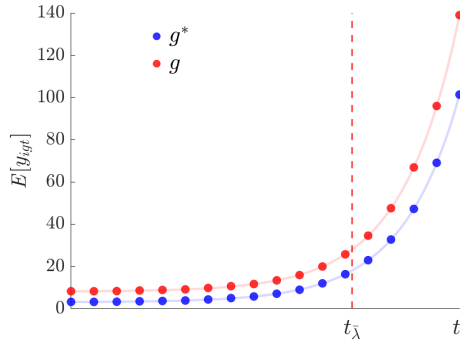
(b1) CTT:

$$\lambda_t$$



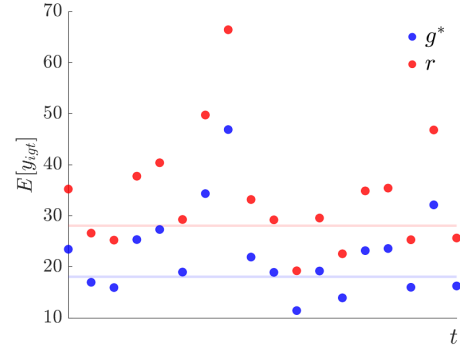
(a2) Outcome Level:

$$\mathbb{E}_i[y_{it}|i \in g]$$



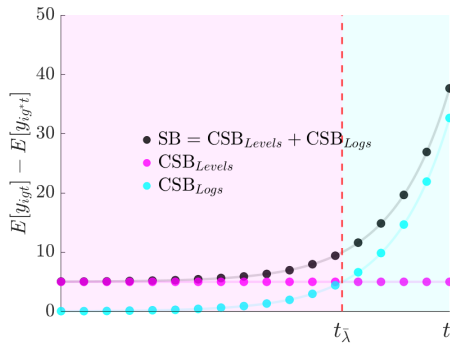
(b2) Outcome Level:

$$\mathbb{E}_i[y_{it}|i \in g]$$



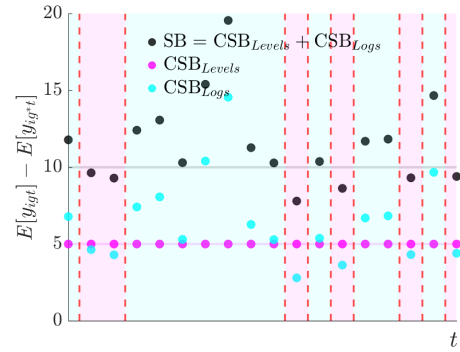
(a3) Selection Bias, in Levels:

$$\mathbb{E}_i[y_{it}|i \in g] - \mathbb{E}_i[y_{it}|i \in g^*]$$



(b3) Selection Bias, in Levels:

$$\mathbb{E}_i[y_{it}|i \in g] - \mathbb{E}_i[y_{it}|i \in g^*]$$



Notes: $\bar{\theta}_{0g} > \bar{\theta}_{0g^*}$, $\bar{\theta}_{1g} > \bar{\theta}_{1g^*} > 0$; growth rate $l_\lambda > 0$. Panel (a) shows the common component, group-specific outcomes, and selection bias under a deterministic CTT, panel (b) under a stochastic DGP. On the figure axes, $E[y_{igt}]$ is short-hand notation for $\mathbb{E}_i[y_{it}|i \in g]$, and accordingly for group g^* and for the log outcomes.

Proposition 4. Linear Relationship in Outcome-on-Outcome Space: For any two groups, g and g^* , that follow DGP (1), the expected outcome without treatment of one group can be expressed as a linear transformation of the expected outcome of the other group: $\mathbb{E}_i[y_{it}|i \in g] = \omega_0 + \omega_1 \mathbb{E}_i[y_{it}|i \in g^*]$. The coefficients (ω_0, ω_1) are pinned down by differences between the conditional means of the scaling parameters. This linear relationship holds regardless of the evolution over time of the common component λ_t .

Proof. Taking the expectation over individuals in (1) for groups g and g^* , we obtain

$$\mathbb{E}_i[y_{it}|i \in g] = \bar{\theta}_{0g} + \bar{\theta}_{1g}\lambda_t \quad \text{and} \quad \mathbb{E}_i[y_{it}|i \in g^*] = \bar{\theta}_{0g^*} + \bar{\theta}_{1g^*}\lambda_t,$$

with $\bar{\theta}_{0g} = \mathbb{E}_i[\theta_{0i}|i \in g]$ and $\bar{\theta}_{1g} = \mathbb{E}_i[\theta_{1i}|i \in g]$ and likewise for g^* .

Combining the two equations yields

$$\mathbb{E}_i[y_{it}|i \in g^*] = \omega_0 + \omega_1 \mathbb{E}_i[y_{it}|i \in g] \tag{11}$$

where $\omega_0 = \bar{\theta}_{0g^*} - \frac{\bar{\theta}_{1g^*}}{\bar{\theta}_{1g}} \bar{\theta}_{0g}$ and $\omega_1 = \frac{\bar{\theta}_{1g^*}}{\bar{\theta}_{1g}}$.

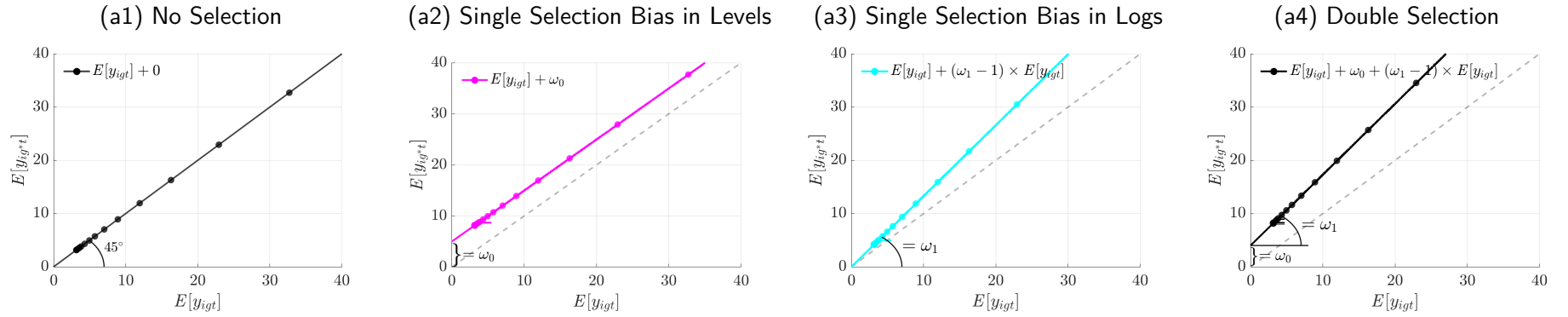
□

As emphasized in the introduction, this outcome-on-outcome representation directly links our approach to a large literature built around the synthetic control approach (Abadie and Gardeazabal, 2003; Abadie et al., 2010). A fixed effect at the group level is present in our setting—which lends more flexibility to the SC approach (as in Arkhangelsky et al., 2021; Doudchenko and Imbens, 2016). Importantly, intercept ω_0 not only captures group differences in the level shifter, i.e., level fixed effects, but in addition is also shaped by group differences in the proportional shifters. This transparently follows from the interactive fixed effect structure of our DGP.

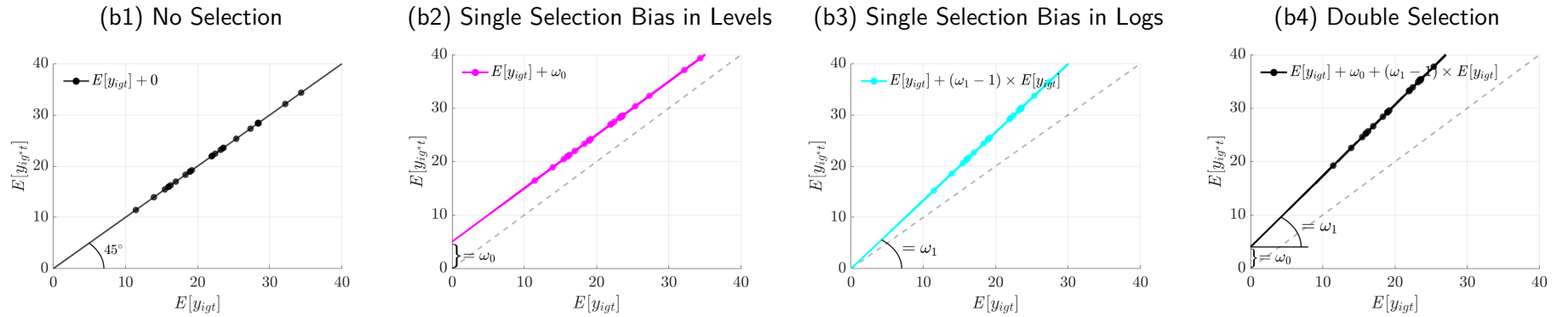
Figure 4 illustrates how the outcome-on-outcome mapping reflects the presence and type of selection bias between groups for all four cases outlined in Table 1. The two panels of the figure reflect two different examples for the common component: in panel (a) λ_t features a strong deterministic component growing over time, while in panel (b) λ_t is purely stochastic. In the second case, the randomness of the common component gives rise to selection bias which evolves non-monotonically over time, as we discussed before. However, regardless of the evolution of λ_t over time, in all four different cases of selection bias, the outcome-on-outcome relationship remains exactly linear, as implied by Proposition 4.

Figure 4: Visualization of Double Selection in the Outcome-on-Outcome Space: $\mathbb{E}_i[y_{it}|i \in g^*]$ against $\mathbb{E}_i[y_{it}|i \in g]$

(a) A Deterministic CTT: $\lambda_t = \lambda_0(1 + I_\lambda)^t$



(b) A Stochastic CTT: $\lambda_t = \bar{\lambda}e^{\eta_t}$ and $\eta_t \sim iid(0, \sigma_\eta^2)$



Notes: In panels (a) and (b), the four figures show the linear relationship between the group-level average outcomes resulting under four different cases of selection bias. In the examples shown, we have $\bar{\theta}_{0g} \geq \bar{\theta}_{0g^*} \geq 0$, $\bar{\theta}_{1g} \geq \bar{\theta}_{1g^*} > 0$. On the figure axes, $E[y_{igt}]$ is short-hand notation for $\mathbb{E}_i[y_{it}|i \in g]$, and accordingly for group g^* and for the log outcomes.

In the absence of selection ($\bar{\theta}_{0g} = \bar{\theta}_{0g^*} = 0, \bar{\theta}_{1g} = \bar{\theta}_{1g^*}$) expected outcomes for groups g and g^* lie on the 45° line, as shown in panels (a1) and (b1). Under single selection in levels ($\bar{\theta}_{0g} \neq \bar{\theta}_{0g^*}, \bar{\theta}_{1g} = \bar{\theta}_{1g^*}$), the intercept of the linear mapping is non-zero, which shifts the line vertically while preserving the slope. This is depicted in panels (a2) and (b2). Under single selection in logs ($\bar{\theta}_{0g} = \bar{\theta}_{0g^*} = 0, \bar{\theta}_{1g} \neq \bar{\theta}_{1g^*}$), the slope deviates from one, while the intercept remains anchored at zero, shown in Panels (a3) and (b3). Finally, under double selection ($\bar{\theta}_{0g} \neq \bar{\theta}_{0g^*}, \bar{\theta}_{1g} \neq \bar{\theta}_{1g^*}$), as shown in panels (a4) and (b4), the mapping exhibits both a shifted intercept and a non-unit slope.

3 Identification of Policy Under Double Selection Bias

Again, we consider the outcome q_{it} of an individual i belonging to group g at time t . Let $\mathbf{P}_{gt} \in \{0, 1\}$ denote the treatment status of a group g in period t , i.e., $\mathbf{P}_{gt}$ takes on value 1 if in period t group g is subject to the policy of interest, and value 0 otherwise. We consider a setting where no-one is subject to the policy initially, before, starting in some period t_p , a treatment group is subject to a policy change, and a control group is not. The treatment status of group g is captured by a binary variable $\mathbf{P}_g$:

$$\mathbf{P}_g = \begin{cases} 1 & \text{if } \mathbf{P}_{gt} = 0 \forall t < t_p \wedge \mathbf{P}_{gt} = 1 \forall t \geq t_p \\ 0 & \text{if } \mathbf{P}_{gt} = 0 \forall t. \end{cases} \quad (12)$$

We refer to the two groups by $\mathcal{T}$ ($\mathbf{P}_{\mathcal{T}} = 1$) and $\mathcal{C}$ ($\mathbf{P}_{\mathcal{C}} = 0$).

Employing potential outcome notation (cf. [Imbens and Rubin, 2015](#)), we assume that for each possible treatment status there is a potential outcome, which we denote by $q_{it}(1)$ and $q_{it}(0)$, respectively. Our object of interest is the true policy at some time $t \geq t_p$, $q_{it}(1) - q_{it}(0)$. The identification challenge is that we observe an individual only as either treated or untreated. In Assumption 1, we establish a set of three assumptions under which the policy effect is identified from observable data for a set of individuals, some of which are treated and some of which are not, which we state in Theorem 1. Precisely, we are interested in the identification of the Average Treatment Effect on the Treated (ATT) in period t :

$$ATT(t) = \mathbb{E}_i[q_{it}(1) - q_{it}(0) | i \in \mathcal{T}] \text{ for } t \geq t_p, \quad (13)$$

where the expectation operator is with respect to individuals i , taken here over all individuals within the treated group $\mathcal{T}$.

Assumption 1. The DGP of q_{it} satisfies the following three assumptions.

(IA1) *Stable Unit Treatment Value Assumption (SUTVA)*: We assume that the treatment status P_g uniquely pins down which of the two possible potential outcomes is observed for any t , and that the treatment status of any other unit is irrelevant,

$$q_{it} = q_{it}(P_g) \quad \text{for } i \in g. \quad (14)$$

(IA2) *No Pre-Treatment Effects (NPE)*: we assume that for any t before the policy date t_p , the two potential outcomes are the same,

$$\mathbb{E}_i[q_{it}(0)|i \in \mathcal{T}] = \mathbb{E}_i[q_{it}(1)|i \in \mathcal{T}] \quad \text{for } t < t_p. \quad (15)$$

(IA3) *Double Constant Selection Bias (CSB)*: we assume a constant ratio (between groups) of changes of the potential no-treatment outcome,

$$\frac{\mathbb{E}_i[q_{it}(0)|i \in \mathcal{T}] - \mathbb{E}_i[q_{i,t_p-1}(0)|i \in \mathcal{T}]}{\mathbb{E}_i[q_{it}(0)|i \in \mathcal{C}] - \mathbb{E}_i[q_{i,t_p-1}(0)|i \in \mathcal{C}]} = \phi \quad \text{for } t \geq t_p \quad (16)$$

where $\phi \equiv \begin{cases} 1, & \text{if } \forall \tau \in \mathbb{N}^+ : \frac{\mathbb{E}_i[q_{i,t_p-1}(0)|i \in \mathcal{T}] - \mathbb{E}_i[q_{i,t_p-1-\tau}(0)|i \in \mathcal{T}]}{\mathbb{E}_i[q_{i,t_p-1}(0)|i \in \mathcal{C}] - \mathbb{E}_i[q_{i,t_p-1-\tau}(0)|i \in \mathcal{C}]} = 0 \\ \frac{\mathbb{E}_i[q_{i,t_p-1}(0)|i \in \mathcal{T}] - \mathbb{E}_i[q_{i,t_p-1-\tau}(0)|i \in \mathcal{T}]}{\mathbb{E}_i[q_{i,t_p-1}(0)|i \in \mathcal{C}] - \mathbb{E}_i[q_{i,t_p-1-\tau}(0)|i \in \mathcal{C}]} & \text{for some } \tau \in \mathbb{N}^+, \text{ else.} \end{cases}$

Note that if $\phi = 1$, which is the case in a setting with either of the two groups displaying a flat outcome time path before policy, (16) simplifies to the standard assumption of a Common Time Trend in levels.

Theorem 1. Identification of Policy Effect: If a DGP satisfies assumptions (IA1)–(IA3), then the policy effect in (13) (ATT) is identified from observable outcomes of the treated and untreated groups.

Proof. By assumption (IA1), SUTVA, the observed outcome for group $\mathcal{T}$ is equal to the potential outcome under treatment. In order to identify the policy effect in (13) from observables, we need to establish that under assumptions (IA1)–(IA3), the expected no-treatment outcome $\mathbb{E}_i[q_{it}(0)|i \in \mathcal{T}]$ can be replaced by observables.

Starting from (IA3), CSB, gives an expression for $\mathbb{E}_i[q_{igt}(0)]$ for $t \geq t_p$:

$$\mathbb{E}_i[q_{it}(0)|i \in \mathcal{T}] = \mathbb{E}_i[q_{i,t_p-1}(0)|i \in \mathcal{T}] + \phi \left(\mathbb{E}_i[q_{it}(0)|i \in \mathcal{C}] - \mathbb{E}_i[q_{i,t_p-1}(0)|i \in \mathcal{C}] \right) \quad \text{for } t \geq t_p.$$

Before turning to the expression for ϕ , note that by (IA2), NPE, the potential no-treatment outcome of the treated group $\mathcal{T}$ is equal to the treatment outcome for $t < t_p$, which gives

$$\mathbb{E}_i[q_{it}(0)|i \in \mathcal{T}] = \mathbb{E}_i[q_{i,t_p-1}(1)|i \in \mathcal{T}] + \phi \left(\mathbb{E}_i[q_{it}(0)|i \in \mathcal{C}] - \mathbb{E}_i[q_{i,t_p-1}(0)|i \in \mathcal{C}] \right) \text{ for } t \geq t_p.$$

Then, by (IA1), SUTVA, we note that the observed outcomes for the groups $\mathcal{T}$ and $\mathcal{C}$ correspond to the potential outcomes under treatment and no treatment, respectively. This implies that

$$\mathbb{E}_i[q_{it}(0)|i \in \mathcal{T}] = \mathbb{E}_i[q_{i,t_p-1}|i \in \mathcal{T}] + \phi \left(\mathbb{E}_i[q_{it}|i \in \mathcal{C}] - \mathbb{E}_i[q_{i,t_p-1}|i \in \mathcal{C}] \right) \text{ for } t \geq t_p. \quad (17)$$

We can then use the same two assumptions (IA2) and (IA1) in the expression for ϕ , which in the above equation is then given by observable outcomes as

$$\phi = \begin{cases} 1, & \text{if } \forall \tau \in \mathbb{N}^+ : \frac{\mathbb{E}_i[q_{i,t_p-1}|i \in \mathcal{T}] - \mathbb{E}_i[q_{i,t_p-1-\tau}|i \in \mathcal{T}]}{\mathbb{E}_i[q_{i,t_p-1}|i \in \mathcal{C}] - \mathbb{E}_i[q_{i,t_p-1-\tau}|i \in \mathcal{C}]} = 0 \\ \frac{\mathbb{E}_i[q_{i,t_p-1}|i \in \mathcal{T}] - \mathbb{E}_i[q_{i,t_p-1-\tau}|i \in \mathcal{T}]}{\mathbb{E}_i[q_{i,t_p-1}|i \in \mathcal{C}] - \mathbb{E}_i[q_{i,t_p-1-\tau}|i \in \mathcal{C}]} & \text{for some } \tau \in \mathbb{N}^+, \text{ else.} \end{cases}$$

Thus, under (IA1)–(IA3), observable outcomes of groups $\mathcal{T}$ and $\mathcal{C}$ identify the expected outcomes under both, treatment and no treatment, for group $\mathcal{T}$, which identifies the ATT in (13). $\square$

Illustration with Benchmark DGP. For illustration of identification, consider the benchmark DGP of equation (1), which satisfies Assumption 1, and which we repeat here for convenience, using the notation for policy treatment introduced above, i.e., $\mathbf{P}_{gt} = 1$ if group g is treated in period t :

$$q_{it} = \theta_{0i} + \theta_{1i}\lambda_t + \mathbf{P}_{gt}\gamma_{it} + \varepsilon_{it} \quad \text{for } i \in g.$$

With this DGP, the potential outcomes under treatment and no treatment are given by

$$q_{it}(0) = \theta_{0i} + \theta_{1i}\lambda_t + \varepsilon_{it} \quad \text{and} \quad q_{it}(1) = \theta_{0i} + \theta_{1i}\lambda_t + \gamma_{it} + \varepsilon_{it},$$

and thus the ATT in period t is given by

$$ATT(t) = \mathbb{E}_i[q_{it}(1) - q_{it}(0)|i \in \mathcal{T}] = \mathbb{E}_i[\gamma_{it}|i \in \mathcal{T}] \text{ for } t \geq t_p. \quad (18)$$

Again, we denote the conditional means of the group-specific shifters by $\bar{\theta}_{0g} = \mathbb{E}_i[\theta_{0i}|i \in g]$ and $\bar{\theta}_{1g} = \mathbb{E}_i[\theta_{1i}|i \in g]$ for $g \in \{\mathcal{T}, \mathcal{C}\}$. Use the DGP in (17) (i.e., in the expression that replaces

the unobserved no-treatment outcome for the treated group with observables) to obtain

$$\begin{aligned}
\mathbb{E}_i[q_{it}(0)|i \in \mathcal{T}] &= \mathbb{E}_i[q_{i,t_p-1}|i \in \mathcal{T}] + \phi \left(\mathbb{E}_i[q_{it}|i \in \mathcal{C}] - \mathbb{E}_i[q_{i,t_p-1}|i \in \mathcal{C}] \right) \\
&= \bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}}\lambda_{t_p-1} + \phi \left(\bar{\theta}_{0\mathcal{C}} + \bar{\theta}_{1\mathcal{C}}\lambda_t - \bar{\theta}_{0\mathcal{C}} - \bar{\theta}_{1\mathcal{C}}\lambda_{t_p-1} \right) \\
&= \bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}}\lambda_{t_p-1} + \phi\bar{\theta}_{1\mathcal{C}} \left(\lambda_t - \lambda_{t_p-1} \right) \text{ for } t \geq t_p,
\end{aligned} \tag{19}$$

where for $\bar{\theta}_{1\mathcal{T}} \neq 0$ and $\bar{\theta}_{1\mathcal{C}} \neq 0$, ϕ is given by

$$\frac{\bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}}\lambda_{t_p-1} - \bar{\theta}_{0\mathcal{T}} - \bar{\theta}_{1\mathcal{T}}\lambda_{t_p-1-\tau}}{\bar{\theta}_{0\mathcal{C}} + \bar{\theta}_{1\mathcal{C}}\lambda_{t_p-1} - \bar{\theta}_{0\mathcal{C}} - \bar{\theta}_{1\mathcal{C}}\lambda_{t_p-1-\tau}} = \frac{\bar{\theta}_{1\mathcal{T}}\lambda_{t_p-1} - \bar{\theta}_{1\mathcal{T}}\lambda_{t_p-1-\tau}}{\bar{\theta}_{1\mathcal{C}}\lambda_{t_p-1} - \bar{\theta}_{1\mathcal{C}}\lambda_{t_p-1-\tau}} = \frac{\bar{\theta}_{1\mathcal{T}}}{\bar{\theta}_{1\mathcal{C}}}, \tag{20}$$

which back in (19) implies

$$\mathbb{E}_i[q_{igt}(0)|i \in \mathcal{T}] = \bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}}\lambda_{t_p-1} + \frac{\bar{\theta}_{1\mathcal{T}}}{\bar{\theta}_{1\mathcal{C}}}\bar{\theta}_{1\mathcal{C}} \left(\lambda_t - \lambda_{t_p-1} \right) \text{ for } t \geq t_p. \tag{21}$$

Subtracting this from the observed treatment outcome of group $\mathcal{T}$, we obtain that the *ATT* for some $t \geq t_p$ is identified from observables as:

$$\begin{aligned}
ATT(t) &= \bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}}\lambda_t + \mathbb{E}_i[\gamma_{it}|i \in \mathcal{T}] - \bar{\theta}_{0\mathcal{T}} - \bar{\theta}_{1\mathcal{T}}\lambda_{t_p-1} - \frac{\bar{\theta}_{1\mathcal{T}}}{\bar{\theta}_{1\mathcal{C}}}\bar{\theta}_{1\mathcal{C}} \left(\lambda_t - \lambda_{t_p-1} \right) \\
&= \mathbb{E}_i[\gamma_{it}|i \in \mathcal{T}] + \bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}} \left(\lambda_t - \lambda_{t_p-1} \right) - \bar{\theta}_{0\mathcal{T}} - \bar{\theta}_{1\mathcal{T}} \left(\lambda_t - \lambda_{t_p-1} \right) \\
&= \mathbb{E}_i[\gamma_{it}|i \in \mathcal{T}]
\end{aligned}$$

Note that the above described procedure establishes identification for the different cases discussed in Section 2, and thus covers single selection bias, in levels or logs, and double selection bias, which combines the two. While we express the period- t average treatment effect, $ATT(t)$, as additive in (1), we can translate it into a proportional difference. This is similar to the usual ways of expressing an average treatment effect in levels as percentage of the mean value of the control group (e.g., [Chen and Roth, 2024](#)).

4 Estimation

In this section, we introduce an estimator for the average treatment effect on the treated, $ATT(t) = \mathbb{E}_i[\gamma_{it}|i \in \mathcal{T}]$, that builds on the identifying assumptions outlined in the preceding section and proceeds in two steps. In the first step, we use pre-treatment outcome time paths at the group level and recover estimates of the (linear) relationship between those group-level

outcomes as outlined in Equation (11). To be precise, we fit a linear regression of the average outcome of the control group, $\mathbb{E}_i[q_{it}|i \in \mathcal{C}]$, on the average outcome of the treatment group, $\mathbb{E}_i[q_{it}|i \in \mathcal{T}]$. In the second step, we normalize the control group outcomes at the micro-level with the obtained coefficients. With these normalized outcomes for the control group at hand, we then estimate the policy effect within a standard dynamic Difference-in-Differences regression framework. Further, within this second-step regression, we can also resort to otherwise standard approaches to pretesting. Precisely, we can test the null hypothesis of constant double selection in the pre-treatment observables: The normalization ensures that, in expectation, pre-policy outcomes coincide between the two groups. If the level and proportional selection components are indeed constant, then the normalization recovered in the linear regression of the first step works equally well across pre-policy period. This results in a statistically insignificant difference between the conditional means of the normalized outcomes.

A Simple Example. We start with a simple linear example that satisfies (IA1) through (IA3) to illustrate our estimation approach. In this illustration we directly operate at the group level in both steps to focus on the normalization for both level and proportional selection. In Table 2, we consider outcomes for two groups over three time periods: two pre-policy periods and one post-policy period. The treated and control groups share a common component, λ_t , which for each group g translates linearly into the group-level average outcome. In period $t = 3$, the outcome of treatment group $\mathcal{T}$ is subject to the policy, where the policy effect is γ . The group-specific constant $\bar{\theta}_{0g}$ and slope coefficient $\bar{\theta}_{1g}$ induce the two types of selection bias in the form of group-specific level and proportional differences.

Now recall from Proposition 4 and the corresponding visualization in Figure 4 that, given the common time effects λ_t , we can express $\mathbb{E}_i[q_{it}|i \in \mathcal{T}]$ as a linear transformation of $\mathbb{E}_i[q_{it}|i \in \mathcal{C}]$ for every pre-treatment period t :

$$\mathbb{E}_i[q_{it}|i \in \mathcal{T}] = \underbrace{\left(\bar{\theta}_{0\mathcal{T}} - \frac{\bar{\theta}_{1\mathcal{T}}}{\bar{\theta}_{1\mathcal{C}}} \bar{\theta}_{0\mathcal{C}} \right)}_{\omega_{\mathcal{C}0}} + \underbrace{\frac{\bar{\theta}_{1\mathcal{T}}}{\bar{\theta}_{1\mathcal{C}}}}_{\omega_{\mathcal{C}1}} \mathbb{E}_i[q_{it}|i \in \mathcal{C}] \text{ for } t = 1, 2. \quad (22)$$

This gives rise to the first step of the estimator: we can obtain the two coefficients $\omega_{\mathcal{C}0}$ and $\omega_{\mathcal{C}1}$ from a linear regression of $\mathbb{E}_i[q_{it}|i \in \mathcal{T}]$ on $\mathbb{E}_i[q_{it}|i \in \mathcal{C}]$, which can be estimated using ordinary least squares (OLS) as the solution to the minimization problem:

$$\{\omega_{\mathcal{C}0}, \omega_{\mathcal{C}1}\} = \arg \min ||\mathbb{E}_i[q_{it}|i \in \mathcal{T}] - \underbrace{(\omega_{\mathcal{C}0} + \omega_{\mathcal{C}1} \mathbb{E}_i[q_{it}|i \in \mathcal{C}])}_{=\hat{q}_{\mathcal{C}t}}|| \text{ for } t < t_p. \quad (23)$$

Table 2: Example with Two Groups and Three Periods

(a) Outcomes by group over time			
Period \ Group	$t = 1$ (<i>pre-policy</i>)	$t = 2$ (<i>pre-policy</i>)	$t = 3$ (<i>post-policy</i>)
$\mathcal{C}$	$\bar{\theta}_{0\mathcal{C}} + \bar{\theta}_{1\mathcal{C}}\lambda_1$	$\bar{\theta}_{0\mathcal{C}} + \bar{\theta}_{1\mathcal{C}}\lambda_2$	$\bar{\theta}_{0\mathcal{C}} + \bar{\theta}_{1\mathcal{C}}\lambda_3$
$\mathcal{T}$	$\bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}}\lambda_1$	$\bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}}\lambda_2$	$\bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}}\lambda_3 + \gamma$

(b) Normalized outcomes by group over time			
Period \ Group	$t = 1$ (<i>pre-policy</i>)	$t = 2$ (<i>pre-policy</i>)	$t = 3$ (<i>post-policy</i>)
$\mathcal{C}$	$\bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}}\lambda_1$	$\bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}}\lambda_2$	$\bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}}\lambda_3$
$\mathcal{T}$	$\bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}}\lambda_1$	$\bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}}\lambda_2$	$\bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}}\lambda_3 + \gamma$

Notes: the table uses the same notation as in the DGP described in (1). λ_t represents time effects, $\bar{\theta}_{0g}$ and $\bar{\theta}_{1g}$ are level and proportional selection parameters, respectively. There are three periods and only $t = 3$ is post-policy. γ is the treatment effect of interest.

By using *only* pre-policy data, we ensure that $\hat{\omega}$ captures only the selection parameters. Note that the parameters $\omega_{\mathcal{C}0}$ and $\omega_{\mathcal{C}1}$ are identified through variations of average outcomes between the treatment and control groups over pre-policy periods. This is what makes two pre-policy periods the minimum requirement for being able to pin down the selection parameters.

For the second step of the estimator, we then use these obtained coefficients to construct normalized outcomes for the control group for all periods.

$$\tilde{q}_{\mathcal{C}t} = \omega_{\mathcal{C}0} + \omega_{\mathcal{C}1}\mathbb{E}_i[q_{it}|i \in \mathcal{C}]. \quad (24)$$

Using the coefficients that solve the minimization problem, $\omega_{\mathcal{C}0} = (\bar{\theta}_{0\mathcal{T}} - \frac{\bar{\theta}_{1\mathcal{T}}}{\bar{\theta}_{1\mathcal{C}}}\bar{\theta}_{0\mathcal{C}})$ and $\omega_{\mathcal{C}1} = \frac{\bar{\theta}_{1\mathcal{T}}}{\bar{\theta}_{1\mathcal{C}}}$, we obtain $\tilde{q}_{\mathcal{C}t} = (\bar{\theta}_{0\mathcal{T}} - \frac{\bar{\theta}_{1\mathcal{T}}}{\bar{\theta}_{1\mathcal{C}}}\bar{\theta}_{0\mathcal{C}}) + \frac{\bar{\theta}_{1\mathcal{T}}}{\bar{\theta}_{1\mathcal{C}}}\mathbb{E}_i[q_{it}|i \in \mathcal{C}] = \bar{\theta}_{0\mathcal{T}} + \bar{\theta}_{1\mathcal{T}}\lambda_t$. In this example, the normalized outcomes for the control group satisfy $\mathbb{E}_i[\tilde{q}_{it}|i \in \mathcal{C}] = \mathbb{E}_i[q_{it}|i \in \mathcal{T}]$ in every pre-policy period ($t = 1, 2$) by construction. Likewise, the normalized outcome corresponds to the counterfactual outcome of the treatment group under no treatment in period $t = 3$, as shown in the lower panel of Table 2. In other words, the normalization eliminates both level and proportional selection effects,

ensuring that the normalized outcomes for the control group provide a valid counterfactual for the treated group in the absence of treatment—given constant double selection. This illustrates that, after applying the coefficients obtained using only pre-policy time periods, to the post-policy time period ($t = 3$), the difference between the outcome of the treated group and the normalized outcome of the control group pins down the treatment effect γ .

4.1 The Estimator

As discussed above, the estimation proceeds in two steps. The first step estimates parameters ω_0 and ω_1 , which align the control group's outcomes with those of the treated group in the pre-policy period. The second step applies these coefficients to normalize micro-level outcomes for the control group in pre- and post-policy data, and estimates the average treatment effect on the treated group using a standard Dynamic DiD specification on the normalized outcomes.

Step 1: Identification of Selection Bias in Outcomes-on-Outcomes Framework

For a sample of N individuals observed over T periods, where T^{pre} periods are pre-policy, we regress average outcomes of the treatment group on those of the control. Let $\mathbf{q}_g$ denote the vector of length T^{pre} , which collects the group-specific mean outcomes, $\mathbb{E}_i[q_{it}|i \in g]$, for every $t \in \{1, \dots, t_p - 1\}$. The first-step estimation is summarized in the following linear specification:

$$\mathbf{Y} = \mathbf{X}\boldsymbol{\omega} + \boldsymbol{\epsilon}, \quad \text{where } \mathbf{Y} = \mathbf{q}_T, \quad \mathbf{X} = [\mathbf{1}, \mathbf{q}_C], \quad \boldsymbol{\omega} = [\omega_{C0}, \omega_{C1}]'. \quad (25)$$

Here, $\mathbf{X}$ includes a constant and the mean outcomes for the control group, while $\mathbf{Y}$ contains the mean outcomes for the treated group in each pre-policy period, and $\boldsymbol{\epsilon}$ is a vector of error terms. Following common practice in, e.g., synthetic control group approaches, we abstract from errors-in-variables possibly induced by the fact that group-level outcomes are noisy estimates of the true group means—we return to this in Section 4.2. Coefficients $\boldsymbol{\omega}$ can then be estimated by OLS as $\hat{\boldsymbol{\omega}} = (\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'\mathbf{Y}$. They yield an normalization of control group outcomes such that group level outcomes align with the treated group before the policy in the presence of double selection, as illustrated in Section 2.3. This way, the estimation uncovers the constancy of the linear relationship between group-level outcomes induced by the factor structure. Note that factor loadings (i.e., the level shifters θ_{0i} and proportional shifters θ_{1i}) do not need to be estimated—the policy effect estimation in step 2 builds on group-level normalization.

Step 2: Estimation of Policy Effect

We now use the estimated parameters $\hat{\omega}_{C0}$ and $\hat{\omega}_{C1}$ to construct an normalized outcome for all periods. In principle we can do this at the group-level directly, as we are after group-level average treatment effects. However, going back to the micro level and applying the normalization at the individual level allows us to resort to an otherwise standard event study regression framework which will serve two purposes: (i.), it allows us to employ standard techniques for statistical inference, and (ii.), it enables us to use standard approaches to pretesting, which in addition will serve to assess the presence of double selection. In particular, we define the normalized individual-level outcome for individual i by

$$\tilde{q}_{it} \equiv \begin{cases} q_{it} & \text{if } i \in \mathcal{T} \\ \hat{\omega}_{C0} + \hat{\omega}_{C1}q_{it} & \text{if } i \in \mathcal{C} \end{cases}. \quad (26)$$

We then use the normalized outcomes in the following regression:

$$\tilde{q}_{it} = \phi_i + \phi_t + \sum_{t \neq (t_p-1)} \gamma_{\mathcal{T}t} \mathbb{1}_{it} + \epsilon_{it}. \quad (27)$$

The term ϕ_i is a shorthand notation that captures both a global intercept and individual-level deviations from it. Similarly, ϕ_t represents time fixed effects, estimated relative to a base period. Finally, $\mathbb{1}_{it} = \mathbb{1}[t] \times \mathbb{1}[i \in \mathcal{T}]$ denotes a treatment dummy interacted with time fixed effects. As a consequence of the normalization procedure, which addresses the selection in the pre-policy period, the group-specific mean individual fixed effects for the outcomes of the treated group and the normalized outcomes of the control group should be the same. Consequently, the estimates for $\gamma_{\mathcal{T}t}$ are expected to be statistically equal to zero for all $t < t_p - 1$. For $t \geq t_p$, the estimates for $\gamma_{\mathcal{T}t}$ capture the average treatment effect on the treated in each period.

4.2 Testing for Selection and Inference

Test for (Double) Constant Selection Bias. With the estimates at hand, the two first-step coefficients $\hat{\omega} = \{\hat{\omega}_{C0}, \hat{\omega}_{C1}\}$ capture the (double) selection bias. Given the covariance structure of the first-step estimates we can test if these differ significantly from what would be expected under no selection bias. The coefficients under no selection bias are $\hat{\omega}_{C1} = 1$ and $\hat{\omega}_{C0} = 0$. A joint F-test that fails to reject the null hypothesis $\{\omega_{C0}, \omega_{C1}\} = \{0, 1\}$ thus implies *no selection*, i.e., the average outcomes coincide. Rejecting the null of the F-test implies the presence of selection bias. Importantly, as we outlined in Table 1, this can be either level selection, or proportional selection, or both.

While three cases of selection bias are theoretically possible, we cannot distinguish all of them. Having failed to reject the null of no selection, we can assess in a separate test whether proportional selection is present: the null hypothesis $\hat{\omega}_{C1} = 1$ implies that $\bar{\theta}_{1C} = \bar{\theta}_{1T}$, and thus it implies no proportional selection. If this null is not rejected in a t-test, then, given the rejection of the joint test, *level single selection* is present. We can confirm this in a t-test of $\hat{\omega}_{C0} = 0$. If the same test (the null $\hat{\omega}_{C1} = 1$) is rejected, this implies that $\bar{\theta}_{1C} \neq \bar{\theta}_{1T}$, and thus, proportional selection is present. We are then in one of two possible remaining scenarios: there is *proportional single selection or double selection*. Of course, in practice, failure to reject the F-test of no selection can still go hand in hand with insignificant t-tests for the individual parameter tests. Importantly, our estimator works in either of these cases, including the case of no selection.

The next test is a *pretest* which addresses the stability of the selection process over time in the pre-policy periods. As such, it serves to judge whether the assumption of a (double) *constant* selection bias, which is at the core of the second step of the estimation, is warranted. This pretest of *(double) constant selection bias* tests if the selection parameters, $\bar{\theta}_{0g}$ and $\bar{\theta}_{1g}$, remain constant over time. Given the mapping between $(\bar{\theta}_{0g}, \bar{\theta}_{1g})$ and $(\omega_{0g}, \omega_{1g})$, constant selection implies constant “quality” of the group-level normalization across pre-policy periods. This equivalence can be expressed as: $\mathbb{E}_i[\tilde{q}_{it}|i \in \mathcal{C}] = \mathbb{E}_i[q_{it}|i \in \mathcal{T}]$, for $t < t_p$. Consider the pre-policy treatment effects, $\hat{\gamma}_{Tt}$ for $t < t_p - 1$ in (27): If the selection bias is constant, these effects are statistically equal to zero, indicating no residual differences between the treated and normalized control groups in the pre-policy period. As we are employing standard tests for pre-trends, the power of our test increases with the number of pre-policy periods $t < t_p$. We note that recent research has documented that such pretests tend to suffer from low power (Roth, 2022; Rambachan and Roth, 2023). The same words of caution apply to our test for constant double selection.

Inference. In the context of our two-step estimator, each step contains a challenge for inference. The challenge in the first-step regression of outcomes on outcomes is that the estimates of $\hat{\omega}_0$ and $\hat{\omega}_1$ are identified solely through time variation in outcomes across groups. Standard inference procedures are not well-suited for estimating the standard errors in this context, because they overlook the fact that each observed group-level outcome represents an average across individuals within a time period and thus has its own sampling distribution. The additional challenge in the second-step estimation of the policy effect $\hat{\gamma}_{Tt}$ is that it inherits the sampling uncertainty from the first-step estimation in the predicted normalized outcomes $\hat{\tilde{q}}_{it}$. To address this double challenge, we consider a non-parametric bootstrapping procedure. As our two-step estimator is implemented by two sequential OLS estimations, it is not computationally expensive, which renders the bootstrap approach feasible in practice.

Our proposed bootstrap procedure consists of performing the following steps M times: (i) construct bootstrap sample m by resampling with replacement from the observed outcomes q_{it} within each time period t and group g , denote the bootstrap sample specific outcomes by q_{it}^m .⁴ (ii) compute the average outcome for each group and time, $q_{gt}^m = \frac{\sum_{i \in g} q_{it}^m}{n_{gt}^m}$, where n_{gt}^m is the number of individuals in group g at time t ; (iii) estimate ω_0^m and ω_1^m ; then normalize the outcome for the control group following the procedure outlined in (26); and (iv) use the normalized outcomes to estimate the policy effect, γ_t^m . Note that (ii)–(iv) simply describe the estimation of the two-step estimator for each bootstrap sample. The estimates $\hat{\omega}_0^m$, $\hat{\omega}_1^m$, and $\hat{\gamma}_t^m$ are stored. The (joint) distribution of $\hat{\omega}^m$ then forms the foundation for the selection tests described in the previous section. Likewise, the distribution of $\hat{\gamma}_t^m$ serves to perform the pretest and to assess significance of the estimated policy effects.

5 Empirical Proof of Concept: Revisiting Card (1992)

The objective of this Section is to illustrate how our framework operationally resolves the tension surrounding the choice of outcome units between levels and logs in a setting where economic theory does not uniquely dictate whether selection operates on an additive or a proportional shifter. For this diagnostic exercise, we leverage the classic empirical environment of policy-induced wage changes, analyzing state-level wage distributions for retail sector workers around the July 1988 California minimum wage intervention (Card, 1992). This environment serves as an ideal testing ground for two reasons: first, it features a highly scrutinized treatment effect that allows us to evaluate the accuracy of our estimator against historical baselines, and second, it contains severe cross-state heterogeneity in wage time-paths that violates single-selection frameworks.

The treatment group consists of retail sector workers in California, while the baseline control group follows the design of Card (1992). We use microdata from the Current Population Survey (specifically, the Outgoing Rotation Groups) for the years 1984–1991 (Flood et al., 2026). 1991 is the last year of the analysis, because in April of that year the federal minimum wage rose to be en par with the one in California. 1984 is the first year of the analysis, because of potentially unreliable earnings related variables in the preceding years (as reported in the IPUMS documentation). We use the pre-policy years 1984–1987 to estimate normalization parameters in the first step, and the post-policy years 1989 and 1990 to estimate the treatment effect in the second step.

⁴For panel data, we recommend an alternative resampling strategy: rather than resampling within periods and groups, we suggest resampling entire individual histories. This form of block bootstrapping better preserves the dependence structure in the data as it accounts for the within-individual autocorrelation structure.

We then consider all US states separately as possible controls for California one-by-one. For each possible case, we first check for the presence of double (or single) selection bias of California versus the potential control group. We then explore the constancy of the uncovered selection bias in the pre-policy years, and estimate the treatment effect under the assumption of constant selection bias. This exploration yields a range of states as valid controls in the presence of (estimated) double selection bias, which in turn leads to the failure of pretests in single selection frameworks. As some states already raise their minimum wage to or above the level of California before the federal minimum wage hike, for each control state, we estimate the treatment effect only for the years before the increase. We use the data provided by [Vaghul and Zipperer \(2016\)](#) to identify the effective state level minimum wage in each year.

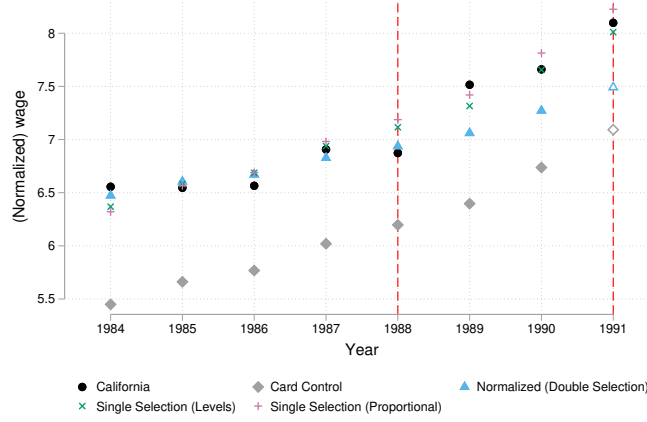
5.1 The [Card \(1992\)](#) Control: A Nested Benchmark

For direct comparability to Card, the first control group for Californian workers are those living in Arizona, Florida, Georgia, New Mexico, or Dallas-Fort Worth. In panel (a) of Figure 5, we show the time series of average wages in California and this *Card control* group. Focus first on the raw outcomes: the black circles show average wages in California, the gray squares show average wages in the Card control group. There is a clear sign of positive selection bias, as the average wages in California are about more than one dollar higher than in the Card control group. Over the first few years, the gap appears to slightly close with the wages in California not moving much before increasing again, and the wages in the Card control steadily growing. The blue diamonds give the normalized outcomes in the Card control group based on the first-step estimates of the double selection framework. These normalized wages effectively coincide with the Californian wages before the policy date. After the policy date one sees California lying above the control, which points to a positive effect of the policy: average wages of workers in the retail sector increase after a rise of the minimum wage.

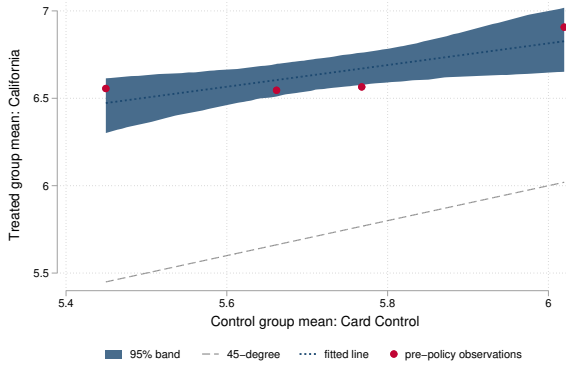
In panel (b), we show the outcome-on-outcome relationship between California and the Card control group for the years before the policy change, i.e., 1984–1987. This relationship informs the first step of our estimator, which linearly regresses the average outcome in California on the average outcome in the Card control group. Before turning to a formal statistical procedure testing for selection, we can gain some insights from a visual inspection of the estimated linear relationship. First, that the outcomes lie above the 45°-line reflects that the wage rate in California lies above the wage rate in the Card control group, which indicates positive selection bias. Second, the difference of the estimated relationship to the 45°-line, while close to being flat mildly closes as wages grow.

Figure 5: Empirical Illustration: Replication of Card (1992)

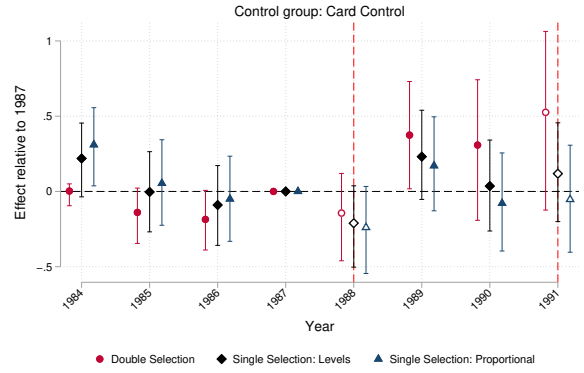
(a) Time Series of Average Wages and Normalized Wages



(b) Step 1: Outcome-on-Outcome Pre-Policy



(c) Step 2: Policy Effect Estimation



Notes: Panel (a) shows the time series of average wages and normalized average wages for California and the Card control group. Panel (b) shows the first-step regression and data points. Panel (c) shows second-step event study regressions for the resulting normalized wage measures under double selection (red circles), single selection in levels (black diamonds) and proportional single selection (blue triangles). The first vertical red dashed line indicates the minimum wage increase in California; the second indicates the federal minimum wage increase. Hollow markers indicate years in which workers in the Card control are subject to the higher minimum wage. Whiskers show 5th and 95th percentiles of 500 bootstraps. The Card control group is comprised of individuals in Arizona, Florida, Georgia, New Mexico, or Dallas-Fort Worth.

The fitted line in (b) has an intercept of $\hat{\omega}_{C0} = 3.09$ ($s.e. = 1.43$), and a slope coefficient of $\hat{\omega}_{C1} = 0.62$ ($s.e. = 0.44$), whereby the standard errors are based on 500 bootstrap draws. Considering 90% bootstrap confidence bands ($5^{th} - 95^{th}$ percentiles), we find that the intercept is significantly away from zero (with the 5^{th} percentile at 0.64). The slope confidence band includes 1 (with the 95^{th} percentile at 1.05). The estimates are reported at the top of Table B.1. Taken together, these estimates support level selection bias between California and the Card control group. However, the unity slope falls into the 90% band just so: it is at the 93.8^{th} percentile of the bootstrap draws, and so it lies outside the 87.6% interval, which means that

proportional selection (in addition to level selection) is only rejected at a confidence level of 12.4%.

We then use the obtained coefficients in the micro panel data and construct normalized wages for individuals living in the Card comparison group regions. In panel (c) of Figure 5, we plot event study regressions capturing step 2 of the estimation based on these normalized wages in red—we also compare it with two alternative wage measures, to which we will turn shortly. All coefficients are relative to baseline year 1987, which is the year before the minimum wage increase in California. The whiskers of the event study plot show the 5th and 95th percentiles of the 500 bootstrap draws. All event study point estimates are also reported in Tables B.2–B.4. We see that in the pre-period (1984–1986) the individual coefficients are all statistically insignificant. This implies that a strict pretest which requires insignificant pre-policy coefficients (Roth, 2022) is passed. Thus, this pretest speaks in favor of the selection bias estimated in step 1 being constant over time in the absence of policy changes. Reflecting the time series of normalized wages, which already point to a positive policy effect, the event study regression recovers positive point estimates of the average treatment effect post policy: For 1989, the estimated effect of \$0.37 is significant, and starting from about \$6.91 in the pre-policy year it amounts to approximately 5.35 percent. The 1990 effect effect of \$0.31 is insignificant.

Our estimate for 1989 corroborates the result of Card’s original analysis, which reports an estimated wage increase by 5 log points. For the direct comparison of this result to ours, several elements regarding the identification and the used data are noteworthy. First, Card does not pursue any pretest. Instead, he argues for the validity of the parallel trends assumption by documenting that the samples of California and the Card Control are similar along a set of observable characteristics. Second, Card compares mean log wages, which through the lens of our double selection framework implicitly comes with a restriction to proportional group differences, i.e., setting the intercept ω_{C0} to zero.⁵ We will thus also consider this restricted version next.

The blue triangle markers in panel (c) of Figure 5 report the event study coefficients for the resulting single selection estimator. Table B.1 also reports the estimate for the slope coefficient. The restricted model attributes all differences to the slope and yields $\hat{\omega}_1^{single} = 1.16$, which allows the restricted estimator to capture the positive wage difference between California and the Card control group. Based on the visual impression one can still argue for parallel trends 1984–1986 of the resulting normalized series. Panel (a) of Figure 5 also plots the normalized wage under the restricted single selection estimator as purple crosses. However, a strict pretest is not passed, as the event study point estimate for 1984 is significant at the 10% level. The

⁵Note that this restriction to the log specification (proportional differences) is at odds with our pretest results. This is an additional insight that we gain from using pre-1987 data, which Card did not use for this analysis.

Table 3: Summary of Event-Study Results by Estimator

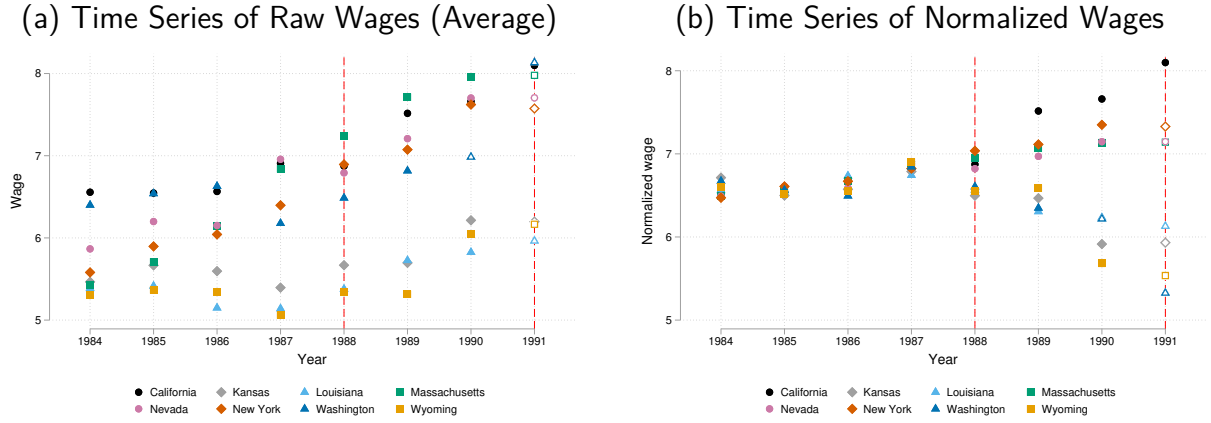
Estimator	Control groups passing pre-test		Average policy effect						
	N	1 st step supports double selection	1989				1990		
			$\bar{\gamma}$ [min, max]	N	N^+	N^-	$\bar{\gamma}$ [min, max]	N	N^+ N^-
Double Selection	27	11	0.656 [0.318, 1.105]	12	12	0	1.061 [0.468, 1.970]	6	6 0
Single Selection (Levels): DiD	27	7	0.454 [-0.582, 0.872]	5	4	1	-0.169 [-0.777, 0.661]	5	2 3
Single Selection (Proportional)	26	6	0.439 [-0.760, 0.976]	4	3	1	-0.347 [-1.086, 0.624]	6	2 4
DiD (Logs)*	24	6	0.065 [-0.083, 0.127]	7	6	1	-0.020 [-0.134, 0.080]	4	2 2

Notes: Table reports number of states for which a pre-test is passed, when using a state as (potential) control for California. For each control state, 500 bootstrap draws implemented. The policy event year 1988 is the year in which the minimum wage in California was raised to \$4.25. Event study effects are relative to baseline year 1987. “Passing pre-test”: every pre-1987 event-study coefficient’s 90% bootstrap CI covers zero. “1st step supports double selection”: the 90% CIs of ω_0 and ω_1 in the double selection estimator exclude 0 and 1 respectively. $\bar{\gamma}$ is the average event-study coefficient for that year over control states that pass the pre-test, whose own 90% CI for that year excludes zero, and whose point estimate lies inside that CI; the range [min, max] of those point estimates is given beneath; N is the number of such states, split into N^+ significantly positive and N^- significantly negative estimates (horizons at or after a state’s own policy change are excluded, so N can differ across years). *: coefficients for the DiD (Logs) row are in log differences, not dollars, and are therefore not comparable in magnitude with the rows above.

post-policy treatment effects are both estimated to be insignificant. This confirms the analysis of the unrestricted double selection estimator, where the point estimates imply a slope smaller than one, such that the model with single proportional selection has a hard time matching the data.

Finally, the black diamond markers in panel (c) of Figure 5 also report estimates under the restriction $\omega_{C1} = 1$ which allow for single selection bias in levels. Unsurprisingly given the first-step estimates of the double selection estimator that statistically support single selection in levels (or: that just weakly point towards double selection), the point estimate of the ATT is not significantly different from the one reported above. In the single selection (levels) estimator, the mild convergence of the Card control group towards California is not taken into account, which one sees visually in the resulting normalized wage series under the single selection estimator, which remains steeper—plotted as green crosses in panel (a) of Figure 5. This is what yields the lower point estimate of the ATT.

Figure 6: Empirical Illustration: Double Selection at Work



Notes: Panels (a) and (b) show time series of average wages and normalized average wages under double selection for individuals in California and in a set of control states, respectively. The first vertical red dashed line indicates the minimum wage increase in California; the second indicates the federal minimum wage increase. Hollow markers indicate years in which a control state is already subject to the higher minimum wage (at the state or federal level).

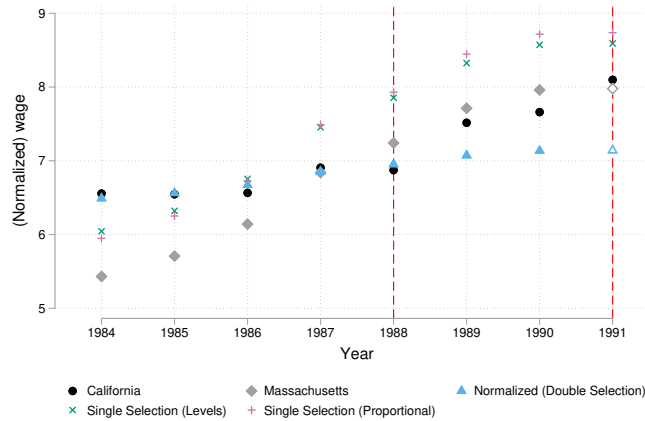
5.2 ATTs Based on Alternative Control States

We now widen the scope of possible control groups for California, and implement the two-step double selection estimator for each US state separately. Table 3 provides a birds-eye perspective on the results. Out of the 50 possible control groups, 27 pass a pretest when considering the normalized wages based on the normalization coefficients estimated in the first step. In eleven of these states, the first-step coefficients statistically support the presence of double selection, i.e., the intercept is significantly different from zero, and the slope is significantly different from one. Table B.1 reports the corresponding estimates for each state.

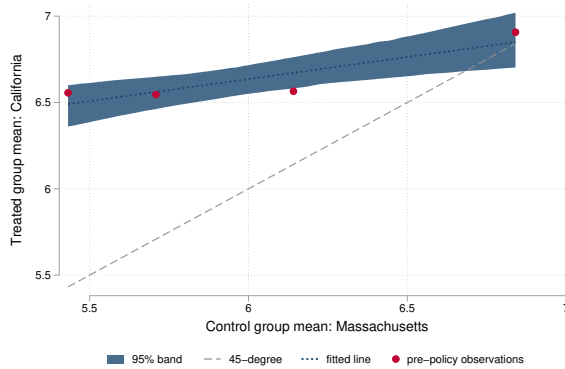
The second main panel of Table 3 summarizes the ATT in 1989 and 1990 averaged over all states for which such a policy effect is estimated to be significant; Table B.2 shows the event-study coefficients separately for each state. Out of the 27 states for which the pretest supports the assumption of constant selection bias, twelve states yield a significant ATT in 1989. The raw average over these twelve is \$0.66: at face value, this suggests that the 1988 minimum wage hike in California led to an increase of hourly wages in the retail sector by \$0.66. While the point estimate based on the Card control is lower (at \$0.37), the \$0.66 is well inside the confidence band of the Card control estimate, such that it is not significantly different. Across the twelve states with a significant effect, the minimum is \$0.31 and the maximum is \$1.97. Similarly, for 1990, six states display a significant ATT conditional on passing the pretest. For these six states, the estimated effect averages at \$1.06 ranging from a minimum of \$0.47 to maximum of \$1.97.

Figure 7: Example of Double Selection: Massachusetts as Control

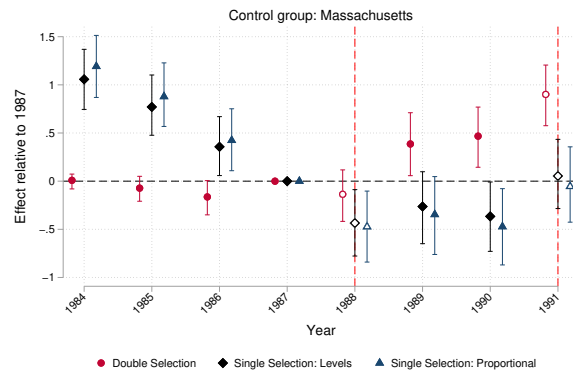
(a) Time Series of Raw Wages (Average) and Normalized Wages



(b) Step 1: Outcome-on-Outcome Pre-Policy



(c) Step 2: Policy Effect Estimation



Notes: Panel (a) shows the time series of average wages and normalized average wages for California and Massachusetts. Panel (b) shows the first-step regression and data points. Panel (c) shows second-step event study regressions for the resulting normalized wage measures under double selection (red circles), single selection in levels (black diamonds) and proportional single selection (blue triangles). The first vertical red dashed line indicates the minimum wage increase in California; the second indicates the federal minimum wage increase. Hollow markers indicate years in which workers in Massachusetts are subject to the higher minimum wage. Whiskers show 5th and 95th percentiles of 500 bootstraps.

Figure 6 shows the outcomes the time series of raw wages and normalized wages for a set of states featuring double selection bias—vis-a-vis workers California—and with significant ATT in 1989. Carefully exploring panel (a) visually, one can see that none of the control states appears to move in tandem—as in parallel time trends—with California over the four pre-policy years. Yet, the double selection that emerges from the first-step estimates implies a time series of normalized wages for each of the states that aligns with California in 1984–1987, as shown in panel (b).

The visual impression is confirmed more clearly when directly comparing the event study plots that permit double selection to the the constrained cases where single selection is imposed

(either proportional or in levels). To illustrate this, we focus on workers in Massachusetts as the control group in Figure 7, where we plot the raw (and normalized) wages together with the event study plots for the three estimators: Double selection, single selection in levels, and single selection in proportions. The double selection estimator picks up that during the pre-policy period Massachusetts closes the gap to California: none of the two single selection (proportional or in level) frameworks can capture this, which results in their pre-period normalized wages not aligned with their Californian counterparts as shown in panel (a). The pre-period outcome-on-outcome regression in panel (b) shows the clear pattern of double selection: an intercept and slope significantly different from zero and unity, respectively. Clearly, the single selection estimators reject parallel time trends in the prepolicy years—see panel (c), whereas the double selection estimator’s normalization picks up the systematic and time-stable difference between California and the control states. The same insights emerge from the conducting direct comparisons using the control states in Figure 6; see Appendix B.⁶

While the added flexibility of our double selection framework uncovers valid counterfactuals where off-the-shelf DiD specifications are rendered not suitable, it does not always do so. As reported in the state-specific estimates of Tables B.1 and B.2, a range of control states do not deliver support for double constant selection bias—which renders the double selection framework not arbitrary. Table 3 also summarizes the results for the nested restricted estimators allowing for either level or proportional single selection bias. The state specific estimation results are reported in Tables B.3 and B.4. The essential take-away is that the number of states which display parallel time trends is much smaller for either of these restricted estimators. Focusing on 1989, the one control state which delivers a negative point estimate under either type of single selection is Hawaii. Note that the pretests for the separate types of selection bias not always clearly favor one or the other type of selection bias—which reflects that the restricted first-step normalizations yield similar fit. Besides Hawaii, the three states delivering a significant policy effect under proportional single selection, conditional on passing the pretest, are New Mexico, Texas, and West Virginia. These states all also pass the pretest under single selection in levels (where in addition Iowa passes). Across the two single selection specifications, the magnitudes of significant effects are the same.

The single selection estimator allowing for level selection is the standard DiD in levels. On the other hand, the single selection estimator with proportional selection only approximately corresponds to the DiD in logs. For completeness we thus also report the standard event study estimates corresponding to the benchmark DiD in logs. The last row of Table 3 reports the

⁶In appendix Figures B.1 and B.2, we report the estimates of the two steps for the remaining states of Figure 6 (and all other federal states).

summary results. Again, these align with the restricted single selection estimator in magnitudes: the point estimates reported here give log differences, which can approximately be compared to the point estimates of the level specification when translating the latter into percentages. The average ATT of \$0.44 in the single selection estimator translates to about 6.4% (as we are starting from \$6.91). This aligns with the 6.5 log points under the DiD in logs. The difference in the number of states reported in the summary table between the DiD in logs and the Single Selection (Proportional) estimator comes from mild difference in statistical significance. The states which additionally qualify in the DiD in logs estimator just so fall out of the confidence band for the ATT in the single selection estimator. All state-specific estimates for the DiD in logs are reported in Table [B.5](#).

6 Extensions of the Double Selection Framework

We now briefly consider two extensions of the framework. The two-step procedure of our proposed estimator requires aggregating to the group level to estimate the normalization factors in step 1, and then going back to the micro level of the data in step 2. At the cost of not pursuing a pre-trend test, one can also uncover an estimate for the treatment effect in the presence of double selection directly in one step at the group level, as we discuss in the first extension. Second, we consider the possibility of allowing for the presence of some control variables and their effect on the outcome.

6.1 One-Step Estimation in Outcome-on-Outcome Space

In Proposition [4](#) we establish the linear relationship between group-level outcomes without treatment that follows under DGP [\(1\)](#). Our benchmark estimator exploits this in a first-step regression, which uses observations from periods before the policy is introduced in the treated group. The separate second step of the estimation then uses the obtained coefficients to normalize micro-level outcomes and implements a dynamic DiD approach using these normalized outcomes. It delivers an estimate of the average treatment effect on the treated separately for each period.

If we are interested in a pooled average treatment effect (i.e., an average over periods of treatment), we also combine the two steps in one outcome-on-outcome regression. In this case, we use the observed outcomes q_{it} and restrict the policy effect to be constant. The DGP becomes

$$q_{it} = \underbrace{\theta_{0i} + \theta_{1i}\lambda_t + \varepsilon_{it}}_{=y_{it}} + \mathbb{1}_{t \geq t_p} \gamma_i.$$

We now take conditional expectations and combine the resulting expressions for two groups: a treated group $\mathcal{T}$ and an untreated group $\mathcal{C}$. This yields

$$\mathbb{E}_i[q_{it}|i \in \mathcal{T}] = \mathbb{1}_{t \neq t_p}(\omega_{C0} + \omega_{C1}\mathbb{E}_i[q_{it}|i \in \mathcal{C}]) + \mathbb{1}_{t \geq t_p}\mathbb{E}_i[\gamma_i|i \in \mathcal{T}] \quad (28)$$

where $\omega_{C0} = \bar{\theta}_{0\mathcal{T}} - \frac{\bar{\theta}_{1\mathcal{T}}}{\bar{\theta}_{1\mathcal{C}}}\bar{\theta}_{0\mathcal{C}}$ and $\omega_{C1} = \frac{\bar{\theta}_{1\mathcal{T}}}{\bar{\theta}_{1\mathcal{C}}}$.

Thus, the ATT $\gamma = \mathbb{E}_i[\gamma_i|i \in \mathcal{T}]$ is identified along with an intercept and slope coefficient, which as before reflect the differences in the level and proportional selection. One important difference to our benchmark two-step estimator is that the intercept and slope in the outcome-on-outcome regression are informed by both pre-policy and post-policy data points.

6.2 Control Variables

We discussed identification and estimation in a setting without additional control variables. Here we illustrate how our framework can account for time-varying control variables. To this end, consider again the untreated outcomes y_{it} in DGP (1). Extend the DGP by a set of controls collected in X_{it} . We can allow for the effect of these controls on the outcome to differ between the two groups $\mathcal{C}$ and $\mathcal{T}$.

$$y_{it} = \theta_{0i} + \theta_{1i}\lambda_t + X_{it}\beta_g + \varepsilon_{it} \quad \text{for } i \in g, \text{ where } g \in \{\mathcal{C}, \mathcal{T}\}. \quad (29)$$

As the common component λ_t is unobserved, direct OLS estimation of (29) will deliver a biased estimate of β_g , so we can again consider the outcome-on-outcome relationship induced by the DGP. Taking expectations by group we obtain

$$\mathbb{E}_i[y_{it}|i \in g] = \bar{\theta}_{0g} + \bar{\theta}_{1g}\lambda_t + \bar{X}_{gt}\beta_g, \text{ where } \bar{X}_{gt} = \mathbb{E}_i[X_{it}|i \in g]. \quad (30)$$

We can then combine the expressions for two groups to obtain

$$\mathbb{E}_i[y_{it}|i \in \mathcal{T}] = \omega_{C0} + \omega_{C1}\mathbb{E}_i[y_{it}|i \in \mathcal{C}] + \beta_1\bar{X}_{\mathcal{T}t} + \beta_2\bar{X}_{\mathcal{C}t} \quad (31)$$

where $\omega_{C0} = \bar{\theta}_{0\mathcal{T}} - \frac{\bar{\theta}_{1\mathcal{T}}}{\bar{\theta}_{1\mathcal{C}}}\bar{\theta}_{0\mathcal{C}}$, $\omega_{C1} = \frac{\bar{\theta}_{1\mathcal{T}}}{\bar{\theta}_{1\mathcal{C}}}$, $\beta_1 = \beta_{\mathcal{T}}$, and $\beta_2 = -\frac{\bar{\theta}_{1\mathcal{T}}}{\bar{\theta}_{1\mathcal{C}}}\beta_{\mathcal{C}}$.

Upon estimation of this first-stage regression, we can construct normalized residual outcomes at the individual level net of the effect of the control variables as

$$\tilde{q}_{it} \equiv \begin{cases} q_{it} - \hat{\beta}_1 X_{it} & \text{if } i \in \mathcal{T} \\ \hat{\omega}_{C0} + \hat{\omega}_{C1}(q_{it} - \hat{\beta}_C X_{it}), \text{ where } \hat{\beta}_C = -\frac{\hat{\beta}_2}{\hat{\omega}_{C1}} & \text{if } i \in \mathcal{C} \end{cases}. \quad (32)$$

The second-step estimation based on these normalized outcomes then works as before.

7 Conclusion

We began this paper by revisiting a foundational quote from [Angrist and Pischke \(2009\)](#), which highlights the incompatibility of maintaining a constant selection bias simultaneously across levels and logs. Motivated by this scale-dependence tension, we propose a unified framework that interprets an interactive fixed effects model as a *double selection* process. By simultaneously accounting for group-specific level and proportional shifters of an unobserved common time trend, our framework models the evolution of selection bias without forcing an ad hoc transformation of the outcome variable. We show that traditional parallel time trends emerge only as nested, constrained subcases of this more general data-generating process. Our core contribution is establishing that within this unified environment, policy effects remain identifiable under a generalized double constant selection bias restriction.

We provide a simple two-step estimator that operates directly in levels and illustrate the empirical relevance of our framework by revisiting the study by [Card \(1992\)](#) on the California minimum wage expansion. Beyond replicating his original results under a verified nested subcase of single selection, we show that our framework uncovers valid counterfactuals that violate single constant selection bias restrictions but satisfy double selection, enabling policy evaluation even in the absolute absence of a control group with parallel trends. Ultimately, this double selection framework opens up new pathways for panel data econometrics. It reframes a set of convergent or crossing pre-policy trajectories from anomalies to be avoided into informative targets for identification.

References

- Abadie, A., Diamond, A., and Hainmueller, J. (2010). Synthetic control methods for comparative case studies: Estimating the effect of california's tobacco control program. *Journal of the American Statistical Association*, 105(490):493–505.
- Abadie, A. and Gardeazabal, J. (2003). The Economic Costs of Conflict: A Case Study of the Basque Country. *American Economic Review*, 93(1):113–132.
- Angrist, J. D. and Pischke, J.-S. (2009). *Mostly harmless econometrics: An empiricist's companion*. Princeton university press.
- Arkhangelsky, D., Athey, S., Hirshberg, D. A., Imbens, G. W., and Wager, S. (2021). Synthetic difference in differences.
- Athey, S., Bayati, M., Doudchenko, N., Imbens, G., and Khosravi, K. (2018). Matrix completion methods for causal panel data models. Working Paper 25132, National Bureau of Economic Research.
- Athey, S. and Imbens, G. W. (2006). Identification and inference in nonlinear difference-in-differences models. *Econometrica*, 74(2):431–497.
- Bai, J. (2009). Panel data models with interactive fixed effects. *Econometrica*, 77(4):1229–1279.
- Brown, N. L. and Butts, K. (2025). Dynamic treatment effect estimation with interactive fixed effects and short panels. *Journal of Econometrics*, 250:106013.
- Callaway, B. and Karami, S. (2023). Treatment effects in interactive fixed effects models with a small number of time periods. *Journal of Econometrics*, 233(1):184–208.
- Card, D. (1992). Do Minimum Wages Reduce Employment? A Case Study of California, 1987–89. *ILR Review*, 46(1):38–54.
- Chen, J. and Roth, J. (2024). Logs with Zeros? Some Problems and Solutions. *The Quarterly Journal of Economics*, 139(2):891–936.
- Cunningham, S. (2021). *Causal inference: The mixtape*. Yale university press.
- Doudchenko, N. and Imbens, G. W. (2016). Balancing, regression, difference-in-differences and synthetic control methods: A synthesis. Working Paper 22791, National Bureau of Economic Research.

- Finkelstein, A. (2007). The aggregate effects of health insurance: Evidence from the introduction of medicare. *The quarterly journal of economics*, 122(1):1–37.
- Flood, S., King, M., Rodgers, R., Ruggles, S., Warren, J. R., Backman, D., Chen, A., Cooper, G., Richards, S., Schouweiler, M., and Westberry, M. (2026). Integrated public use microdata series, current population survey: Version 13.0 [dataset].
- Gobillon, L. and Magnac, T. (2016). Regional policy evaluation: Interactive fixed effects and synthetic controls. *The Review of Economics and Statistics*, 98(3):535–551.
- Imbens, G. W. and Rubin, D. B. (2015). *Causal Inference for Statistics, Social, and Biomedical Sciences: An Introduction*. Cambridge University Press.
- Kahn-Lang, A. and Lang, K. (2020). The promise and pitfalls of differences-in-differences: Reflections on 16 and pregnant and other applications. *Journal of Business & Economic Statistics*, 38(3):613–620.
- Lechner, M., Rodriguez-Planas, N., and Fernández Kranz, D. (2016). Difference-in-difference estimation by fe and ols when there is panel non-response. *Journal of Applied Statistics*, 43(11):2044–2052.
- McConnell, B. (2024). Can't See the Forest for the Logs: On the Perils of Using Difference-in-Differences With a Log-Dependent. Working papers, City University of London.
- Meyer, B. D. (1995). Natural and quasi-experiments in economics. *Journal of business & economic statistics*, 13(2):151–161.
- Park, S. and Powell, D. (2021). Is the rise in illicit opioids affecting labor supply and disability claiming rates? *Journal of Health Economics*, 76:102430.
- Powell, L. M. and Leider, J. (2020). The impact of seattle's sweetened beverage tax on beverage prices and volume sold. *Economics & Human Biology*, 37:100856.
- Rambachan, A. and Roth, J. (2023). A More Credible Approach to Parallel Trends. *The Review of Economic Studies*.
- Roth, J. (2022). Pretest with caution: Event-study estimates after testing for parallel trends. *American Economic Review: Insights*, 4(3):305–22.
- Roth, J. and Sant'Anna, P. H. C. (2023). When Is Parallel Trends Sensitive to Functional Form? *Econometrica*, 91(2):737–747.

Vaghul, K. and Zipperer, B. (2016). Historical state and sub-state minimum wage data. Working paper, Washington Center for Equitable Growth, Washington, DC.

Appendix

Notation table

Symbol	Definition
q_{it}	Observed outcome for individual i at time t
y_{it}	Counterfactual (pre-policy) outcome, i.e., q_{it} for $t < t_p$
λ_t	Common time trend
θ_{0i}	Individual-specific level shifter
θ_{1i}	Individual-specific proportional (log) shifter
ϵ_{it}	Idiosyncratic shock, $E[\epsilon_{it}] = 0$, $\text{Var}(\epsilon_{it}) = \sigma^2$
u_{it}	Rescaled error: $1 + \frac{\epsilon_{it}}{\theta_{0i} + \theta_{1i}\lambda_t}$
g^*	Reference group (baseline)
$E[y_{it}]$	Expected pre-policy outcome in levels
$E[\ln y_{it}]$	Expected pre-policy outcome in logs
SB in levels	$E[y_{it} i \in g] - E[y_{it} i \in g^*]$
SB in logs	$E[\ln y_{it} i \in g] - E[\ln y_{it} i \in g^*]$
$\Theta_0 C_0$	Set of θ_0 vectors satisfying constraint C_0 : $\theta_{0i} = 0$
$\Theta_1 C_1$	Set of θ_1 vectors satisfying constraint C_1 : $\theta_{1i} = \bar{\theta}_1$
$\bar{\lambda}$	Threshold value at which dominance between selection types switches

A Appendix for Section 2

A.1 Proof of Lemma 1

Proof. (a) Isolating u_{it} from (2) and substituting y_{it} from (1), we obtain:

$$u_{it} = \frac{y_{it}}{\theta_{0i} + \theta_{1i}\lambda_t} = \frac{\theta_{0i} + \theta_{1i}\lambda_t + \epsilon_{it}}{\theta_{0i} + \theta_{1i}\lambda_t} = 1 + \frac{\epsilon_{it}}{\theta_{0i} + \theta_{1i}\lambda_t}.$$

Furthermore, given mean independence of the error term, $\mathbb{E}[\epsilon_{it}|\theta_{0i}, \theta_{1i}, \lambda_t] = 0$, the expected value of u_{it} conditional on $i \in g$ and conditional on the period- t common factor λ_t is:

$$\mathbb{E}_i[u_{it}|i \in g] = 1 + \mathbb{E}_i\left[\frac{\epsilon_{it}}{\theta_{0i} + \theta_{1i}\lambda_t}|\lambda_t, i \in g\right] = 1 + \mathbb{E}_i\left[\frac{1}{\theta_{0i} + \theta_{1i}\lambda_t}\mathbb{E}[\epsilon_{it}|\theta_{0i}, \theta_{1i}, \lambda_t]|i \in g\right] = 1.$$

(b) Let $\ln(u_{it}) = \ln(1 + (u_{it} - 1))$. Since $\mathbb{E}_i[u_{it}|i \in g] = 1$ (Lemma 1(b)), we Taylor expand $\ln(u_{it})$ around $u_{it} = 1$:

$$\ln(u_{it}) = (u_{it} - 1) - \frac{(u_{it} - 1)^2}{2} + \frac{(u_{it} - 1)^3}{3} - \dots$$

Taking conditional expectations on both sides gives:

$$\mathbb{E}_i[\ln(u_{it})|i \in g] = \mathbb{E}_i[u_{it}(t) - 1|i \in g] - \frac{1}{2}\mathbb{E}_i[(u_{it} - 1)^2|i \in g] + \frac{1}{3}\mathbb{E}_i[(u_{it} - 1)^3|i \in g] - \dots$$

For small values of $u_{it} - 1$, the second-order and higher-order terms (i.e., $\mathbb{E}_i[(u_{it} - 1)^2|i \in g]$, $\mathbb{E}_i[(u_{it} - 1)^3|i \in g]$, etc.) become negligible and we can consider the first-order approximation:⁷

$$\mathbb{E}_i[\ln(u_{it})|i \in g] \approx \mathbb{E}_i[u_{it} - 1|i \in g].$$

□

Lemma A.1. *If the common component is constant, i.e., $\lambda_t = \lambda < \infty \forall t$, then the DGP in (1) exhibits constant selection bias (CSB) in both levels (3) and logs (4).*

A.2 Proof of Proposition 1.

Proof. We prove (a) and (b) separately:

(a) Plug (C1) into (3) and (4), respectively:

$$\text{Levels: } \mathbb{E}_i[y_{it}|i \in g] - \mathbb{E}_i[y_{it}|i \in g^*] = \mathbb{E}[\theta_{0i}|i \in g] - \mathbb{E}[\theta_{0i}|i \in g^*] \quad (\text{constant}) \quad (33)$$

$$\begin{aligned} \text{Logs: } & \mathbb{E}_i[\ln y_{it}|i \in g] - \mathbb{E}_i[\ln y_{it}|i \in g^*] \\ & \approx \mathbb{E}_i[\ln(\theta_{0i} + \bar{\theta}_1 \lambda_t)|i \in g] - \mathbb{E}_i[\ln(\theta_{0i} + \bar{\theta}_1 \lambda_t)|i \in g^*] \quad (\text{not constant}) \end{aligned} \quad (34)$$

Thus, the DGP displays a CSB in levels, see Equation (33). The log counterpart of the CSB in levels—i.e., the selection bias term in logs in Equation (4) under restriction (C1), which leads to CSB in levels—is generally not constant (34), except if λ_t is constant.

(b) Plug (C0) into (3) and (4), respectively:

$$\text{Levels: } \mathbb{E}_i[y_{it}|i \in g] - \mathbb{E}_i[y_{it}|i \in g^*] = (\mathbb{E}[\theta_{1i}|i \in g] - \mathbb{E}[\theta_{1i}|i \in g^*])\lambda_t \quad (\text{not constant}) \quad (35)$$

$$\begin{aligned} \text{Logs: } & \mathbb{E}_i[\ln y_{it}|i \in g] - \mathbb{E}_i[\ln y_{it}|i \in g^*] \\ & \approx \mathbb{E}[\ln \theta_{1i}|i \in g] - \mathbb{E}[\ln \theta_{1i}|i \in g^*] \quad (\text{constant}) \end{aligned} \quad (36)$$

Thus, the DGP displays a CSB in logs, see Equation (36). The level counterpart of the CSB in logs—i.e., the selection bias term in levels in Equation (3) under restriction (C0), which leads to CSB in logs—is generally not constant (35), except if λ_t is constant.

⁷ The same argument is implicitly used to approximate growth rates with log differences when the growth rate r is small. Precisely, let $\ln y_{t+1} - \ln y_t = \ln y_t(1 + r_t) - \ln y_t = \ln(1 + r_t) \approx r_t$, thus: $\mathbb{E}[\ln y_{t+1} - \ln y_t] = \mathbb{E}[\ln(1 + r_t)] \approx \mathbb{E}[r_t]$.

□

A.3 Proof of Proposition 2.

Proof. We prove (a) and (b) separately:

(a) The selection bias in levels (3) can be rewritten as:

$$\begin{aligned}
 \text{Levels: } & \mathbb{E}_i[y_{it}|i \in g] - \mathbb{E}_i[y_{it}|i \in g^*] \\
 &= \underbrace{\mathbb{E}[\theta_{0i}|i \in g] - \mathbb{E}[\theta_{0i}|i \in g^*]}_{= \text{CSB in Levels (33)}} + \underbrace{(\mathbb{E}[\theta_{1i}|i \in g] - \mathbb{E}[\theta_{1i}|i \in g^*]) \lambda_t}_{= \text{Level counterpart of CSB in Logs (35)}} \quad (\text{not constant})
 \end{aligned} \tag{37}$$

where the first term captures the CSB in levels (33), while the second term is the level counterpart of the CSB in logs (35).

(b) The selection bias in logs (4) can be rewritten as:

$$\begin{aligned}
 \text{Logs: } & \mathbb{E}_i[\ln y_{it}|i \in g] - \mathbb{E}_i[\ln y_{it}|i \in g^*] \\
 & \approx \underbrace{\mathbb{E}_i[\ln(\theta_{0i} + \bar{\theta}_1 \lambda_t)|i \in g] - \mathbb{E}_i[\ln(\theta_{0i} + \bar{\theta}_1 \lambda_t)|i \in g^*]}_{= \text{Log counterpart of CSB in Levels (34)}} \\
 & + w(\theta_0, \theta_1, \lambda_t) \underbrace{(\mathbb{E}[\ln \theta_{1i}|i \in g] - \mathbb{E}[\ln \theta_{1i}|i \in g^*])}_{= \text{CSB in Logs (36)}} \quad (\text{not constant})
 \end{aligned} \tag{38}$$

for $\bar{\theta}_1 = \mathbb{E}[\theta_{1i}|i \in g^*]$. The first term is the log counterpart of the CSB in levels (34), while the second term is the CSB in logs (36) normalized with weight

$$w(\theta_0, \theta_1, \lambda_t) = \frac{\mathbb{E}_i[\ln(\theta_{0i} + \theta_{1i} \lambda_t)|i \in g] - \mathbb{E}_i[\ln(\theta_{0i} + \bar{\theta}_1 \lambda_t)|i \in g]}{\mathbb{E}[\ln \theta_{1i}|i \in g] - \mathbb{E}[\ln \theta_{1i}|i \in g^*]}.$$

The expression in (38) generally is an approximation as the stated conditional mean of the log outcome in group g^* is approximated as:

$$\mathbb{E}_i[\ln(\theta_{0i} + \theta_{1i} \lambda_t)|i \in g^*] \approx \mathbb{E}_i[\ln(\theta_{0i} + \bar{\theta}_1 \lambda_t)|i \in g^*].$$

In case of degenerate distributions of the shifters within group g^* , i.e., for $\theta_{1i} = \bar{\theta}_1 \forall i \in g^*$ and $\theta_{0i} = \bar{\theta}_0 \forall i \in g^*$, this approximation is exact: $\mathbb{E}_i[\ln(\bar{\theta}_0 + \bar{\theta}_1 \lambda_t)|i \in g^*] = \ln(\bar{\theta}_0 + \bar{\theta}_1 \lambda_t)$.

□

A.4 Characterization of the Selection Bias

Lemmas 2 and 3 establish the link between λ_t and the selection bias. We then use these Lemmas in the proof of Proposition 3.

A.4.1 Proof of Lemma 2

Proof.

(a) Take the first derivative of SB in levels:

$$\begin{aligned} & \frac{\partial \mathbb{E}_i[y_{it}|i \in g] - \mathbb{E}_i[y_{it}|i \in g^*]}{\partial \lambda} = \\ & \frac{\partial (\mathbb{E}[\theta_{0i}|i \in g] + \mathbb{E}[\theta_{1i}|i \in g]\lambda) - (\mathbb{E}[\theta_{0i}|i \in g^*] + \mathbb{E}[\theta_{1i}|i \in g^*]\lambda)}{\partial \lambda} = \\ & \frac{\partial (\mathbb{E}[\theta_{1i}|i \in g] - \mathbb{E}[\theta_{1i}|i \in g^*]) \lambda}{\partial \lambda} = (\mathbb{E}[\theta_{1i}|i \in g] - \mathbb{E}[\theta_{1i}|i \in g^*]) \geq 0 \\ & \Leftrightarrow \mathbb{E}[\theta_{1i}|i \in g] \geq \mathbb{E}[\theta_{1i}|i \in g^*]. \end{aligned}$$

Thus, the selection bias in levels monotonically increases (decreases) in λ if on average the slope coefficient is greater (smaller) in group g than in group g^* . If there is no proportional selection bias, i.e., if $\mathbb{E}[\theta_{1i}|i \in g] = \mathbb{E}[\theta_{1i}|i \in g^*]$, then the selection bias in levels is constant.

(b) Take the first derivative of SB in logs:

$$\begin{aligned} & \frac{\partial \mathbb{E}_i[\ln y_{it}|i \in g] - \mathbb{E}_i[\ln y_{it}|i \in g^*]}{\partial \lambda} = \\ & \frac{\partial \mathbb{E}_i[\ln(\theta_{0i} + \theta_{1i}\lambda)|i \in g] - \mathbb{E}_i[\ln(\theta_{0i} + \theta_{1i}\lambda)|i \in g^*]}{\partial \lambda} = \\ & \mathbb{E}_i \left[\theta_{1i} \frac{1}{\theta_{0i} + \theta_{1i}\lambda} | i \in g \right] - \mathbb{E}_i \left[\theta_{1i} \frac{1}{\theta_{0i} + \theta_{1i}\lambda} | i \in g^* \right] = \\ & \mathbb{E}_i \left[\frac{1}{\frac{\theta_{0i}}{\theta_{1i}} + \lambda} | i \in g \right] - \mathbb{E}_i \left[\frac{1}{\frac{\theta_{0i}}{\theta_{1i}} + \lambda} | i \in g^* \right] \geq 0 \Leftrightarrow \left(\frac{\theta_{0i}}{\theta_{1i}} \right) | g \prec_{FOSD} \left(\frac{\theta_{0i}}{\theta_{1i}} \right) | g^*. \end{aligned}$$

Thus, the selection bias in logs monotonically increases (decreases) in λ if the ratio of the level shifter θ_{0i} and the proportional shifter θ_{1i} in group g^* first-order stochastically dominates the same ratio in group g . Under degenerate distributions, this simplifies to $\left(\frac{\mathbb{E}_i[\theta_{0i}|i \in g]}{\mathbb{E}_i[\theta_{1i}|i \in g]} \right) = \left(\frac{\bar{\theta}_{0g}}{\bar{\theta}_{1g}} \right) \leq \left(\frac{\bar{\theta}_{0g^*}}{\bar{\theta}_{1g^*}} \right) = \left(\frac{\mathbb{E}_i[\theta_{0i}|i \in g^*]}{\mathbb{E}_i[\theta_{1i}|i \in g^*]} \right)$.

□

A.4.2 Proof of Lemma 3

Proof.

(a) Consider the SB in levels:

$$\Delta_{levels} = \left(\mathbb{E}_i[\theta_0|i \in g] - \mathbb{E}_i[\theta_0|i \in g^*] \right) + \left(\mathbb{E}_i[\theta_1|i \in g] - \mathbb{E}_i[\theta_1|i \in g^*] \right) \lambda.$$

Taking limits with respect to λ directly yields:

$$\begin{aligned}\lim_{\lambda \rightarrow 0} \Delta_{levels} &= \left(\mathbb{E}_i[\theta_{0i}|i \in g] - \mathbb{E}_i[\theta_{0i}|i \in g^*] \right) \\ \lim_{\lambda \rightarrow \infty} \Delta_{levels} &= \left(\mathbb{E}_i[\theta_{0i}|i \in g] - \mathbb{E}_i[\theta_{0i}|i \in g^*] \right) + \left(\mathbb{E}_i[\theta_{1i}|i \in g] - \mathbb{E}_i[\theta_{1i}|i \in g^*] \right) \lim_{\lambda \rightarrow \infty} \lambda \\ &= \begin{cases} \infty, & \text{if } \mathbb{E}_i[\theta_{1i}|i \in g] > \mathbb{E}_i[\theta_{1i}|i \in g^*] \\ -\infty, & \text{if } \mathbb{E}_i[\theta_{1i}|i \in g] < \mathbb{E}_i[\theta_{1i}|i \in g^*] \end{cases}.\end{aligned}$$

(b) Consider the SB in logs:

$$\Delta_{logs} = \mathbb{E}_i[\ln(\theta_{0i} + \theta_{1i}\lambda) | i \in g] - \mathbb{E}_i[\ln(\theta_{0i} + \theta_{1i}\lambda) | i \in g^*]$$

Taking limits for λ going to 0 directly gives

$$\lim_{\lambda \rightarrow 0} \Delta_{logs} = \mathbb{E}_i[\ln(\theta_{0i}) | i \in g] - \mathbb{E}_i[\ln(\theta_{0i}) | i \in g^*].$$

Before taking the upper limit, note that we can write Δ_{logs} as $\Delta_{logs} = \mathbb{E}_i[\ln(\theta_{1i}\lambda) + \ln\left(1 + \frac{\theta_{0i}}{\theta_{1i}}\right) | i \in g] - \mathbb{E}_i[\ln(\theta_{1i}\lambda) + \ln\left(1 + \frac{\theta_{0i}}{\theta_{1i}}\right) | i \in g^*]$. Then taking the upper limit we obtain

$$\begin{aligned}\lim_{\lambda \rightarrow \infty} \Delta_{logs} &= \lim_{\lambda \rightarrow \infty} \left(\mathbb{E}_i[\ln(\theta_{1i}\lambda) | i \in g] - \mathbb{E}_i[\ln(\theta_{1i}\lambda) | i \in g^*] \right) \\ &= \lim_{\lambda \rightarrow \infty} \left(\mathbb{E}_i[\ln(\theta_{1i}) + \ln(\lambda) | i \in g] - \mathbb{E}_i[\ln(\theta_{1i}) + \ln(\lambda) | i \in g^*] \right) \\ &= \lim_{\lambda \rightarrow \infty} \left(\mathbb{E}_i[\ln(\theta_{1i}) | i \in g] - \mathbb{E}_i[\ln(\theta_{1i}) | i \in g^*] + \ln(\lambda) - \ln(\lambda) \right) \\ &= \mathbb{E}_i[\ln(\theta_{1i}) | i \in g] - \mathbb{E}_i[\ln(\theta_{1i}) | i \in g^*]\end{aligned}$$

□

A.5 Proof of Proposition 3.

Proof.

- (i) Consider the SB in levels. The existence of the threshold $\bar{\lambda}$ in (9) follows from applying monotonicity of the SB (Lemma 2) and the limiting values of the SB (Lemma 3). The LHS of (9) does not vary with λ , while the RHS is strictly monotonically increasing (or decreasing) in λ_t (Lemma 2) and goes from 0, when λ_t approaches 0, to infinity, when λ_t approaches ∞ (Lemma 3). Therefore, there is a unique $\bar{\lambda}$ at which the RHS equals the constant LHS:

$$\begin{aligned}\left| \mathbb{E}_i[\theta_{0i}|i \in g] - \mathbb{E}_i[\theta_{0i}|i \in g^*] \right| &= \left| (\mathbb{E}_i[\theta_{1i}|i \in g] - \mathbb{E}_i[\theta_{1i}|i \in g^*]) \bar{\lambda} \right| \\ \Leftrightarrow |\bar{\lambda}| &= \frac{\left| \mathbb{E}_i[\theta_{0i}|i \in g] - \mathbb{E}_i[\theta_{0i}|i \in g^*] \right|}{\left| \mathbb{E}_i[\theta_{1i}|i \in g] - \mathbb{E}_i[\theta_{1i}|i \in g^*] \right|}\end{aligned}$$

- (ii) Under degenerate conditional distributions for λ_{0i} and λ_{1i} within each group, the threshold for the SB in levels is $\bar{\lambda} = (|\bar{\theta}_{0g} - \bar{\theta}_{0g^*}|) / (|\bar{\theta}_{1g} - \bar{\theta}_{1g^*}|)$. Consider the SB in logs, which is given by (note that in the degenerate case, Equation (38) holds exactly)

$$\begin{aligned} \mathbb{E}_i[\ln y_{it}|i \in g] - \mathbb{E}_i[\ln y_{it}|i \in g^*] &= (\ln(\bar{\theta}_{0g} + \bar{\theta}_{1g^*}\lambda_t) - \ln(\bar{\theta}_{0g^*} + \bar{\theta}_{1g^*}\lambda_t)) \\ &\quad + w(\theta_0, \theta_1, \lambda_t) (\ln \bar{\theta}_{1g} - \ln \bar{\theta}_{1g^*}) \end{aligned}$$

where

$$w(\theta_0, \theta_1, \lambda_t) = \frac{\ln(\bar{\theta}_{0g} + \bar{\theta}_{1g}\lambda_t) - \ln(\bar{\theta}_{0g} + \bar{\theta}_{1g^*}\lambda_t)}{\ln \bar{\theta}_{1g} - \ln \bar{\theta}_{1g^*}}.$$

The SB in logs is dominated by the selection bias in levels whenever

$$\left| \ln(\bar{\theta}_{0g} + \bar{\theta}_{1g^*}\lambda) - \ln(\bar{\theta}_{0g^*} + \bar{\theta}_{1g^*}\lambda) \right| > \left| w(\theta_0, \theta_1, \lambda) (\ln \bar{\theta}_{1g} - \ln \bar{\theta}_{1g^*}) \right|,$$

and vice versa. Using the definition of $w(\cdot)$, the two are of equal size if

$$\begin{aligned} \left| \ln(\bar{\theta}_{0g} + \bar{\theta}_{1g^*}\lambda) - \ln(\bar{\theta}_{0g^*} + \bar{\theta}_{1g^*}\lambda) \right| &= \left| \ln(\bar{\theta}_{0g} + \bar{\theta}_{1g}\lambda) - \ln(\bar{\theta}_{0g} + \bar{\theta}_{1g^*}\lambda) \right| \\ \Leftrightarrow \left| \ln \left(\frac{\bar{\theta}_{0g} + \bar{\theta}_{1g^*}\lambda}{\bar{\theta}_{0g^*} + \bar{\theta}_{1g^*}\lambda} \right) \right| &= \left| \ln \left(\frac{\bar{\theta}_{0g} + \bar{\theta}_{1g}\lambda}{\bar{\theta}_{0g} + \bar{\theta}_{1g^*}\lambda} \right) \right|. \end{aligned}$$

One of the possible cases satisfying this equality of absolute values is

$$\frac{\bar{\theta}_{0g} + \bar{\theta}_{1g^*}\lambda}{\bar{\theta}_{0g^*} + \bar{\theta}_{1g^*}\lambda} \frac{\bar{\theta}_{0g} + \bar{\theta}_{1g}\lambda}{\bar{\theta}_{0g} + \bar{\theta}_{1g^*}\lambda} = 1.$$

Solving for λ and taking absolute values this gives

$$|\lambda| = \bar{\lambda} = (|\bar{\theta}_{0g} - \bar{\theta}_{0g^*}|) / (|\bar{\theta}_{1g} - \bar{\theta}_{1g^*}|),$$

and thus the threshold obtained for the selection bias in levels also gives a threshold for the selection bias in logs.

□

A.6 Proof of Corollary 2.

Proof. The selection bias in levels between group g and reference group g^* can be written explicitly as:

$$\Delta_{levels} = \mathbb{E}_i[y_{it}|i \in g] - \mathbb{E}_i[y_{it}|i \in g^*] = (\bar{\theta}_{0g} - \bar{\theta}_{0g^*}) + (\bar{\theta}_{1g} - \bar{\theta}_{1g^*})\lambda_t$$

Proof of Part (1): Uniform Dominance (Same-Sign Shifters)

- **Case (1.i):** Suppose $\bar{\theta}_{0g} > \bar{\theta}_{0g^*}$ and $\bar{\theta}_{1g} > \bar{\theta}_{1g^*}$. This implies that $(\bar{\theta}_{0g} - \bar{\theta}_{0g^*}) > 0$ and $(\bar{\theta}_{1g} - \bar{\theta}_{1g^*}) > 0$. Because $\lambda_t > 0$, the product $(\bar{\theta}_{1g} - \bar{\theta}_{1g^*})\lambda_t$ must be strictly positive. The sum of two strictly positive terms is strictly positive, yielding $\Delta_{levels} > 0 \implies \mathbb{E}_i[y_{it}|i \in g] > \mathbb{E}_i[y_{it}|i \in g^*]$ for all λ_t .
- **Case (1.ii):** Suppose $\bar{\theta}_{0g^*} > \bar{\theta}_{0g}$ and $\bar{\theta}_{1g^*} > \bar{\theta}_{1g}$. This implies that $(\bar{\theta}_{0g} - \bar{\theta}_{0g^*}) < 0$ and $(\bar{\theta}_{1g} - \bar{\theta}_{1g^*}) < 0$. With $\lambda_t > 0$, the expression is the sum of two strictly negative components, making $\Delta_{levels} < 0 \implies \mathbb{E}_i[y_{it}|i \in g^*] > \mathbb{E}_i[y_{it}|i \in g]$ uniformly across all periods.

Proof of Part (2): Dominance Reversal (Opposite-Sign Shifters)

Suppose without loss of generality that group g possesses a higher level shifter but a lower proportional shifter, meaning $(\bar{\theta}_{0g} - \bar{\theta}_{0g^*}) > 0$ and $(\bar{\theta}_{1g} - \bar{\theta}_{1g^*}) < 0$. Setting $\Delta_{levels} = 0$ to identify the crossing point yields:

$$(\bar{\theta}_{0g} - \bar{\theta}_{0g^*}) + (\bar{\theta}_{1g} - \bar{\theta}_{1g^*})\bar{\lambda}_s = 0 \implies \bar{\lambda}_s = \frac{\bar{\theta}_{0g} - \bar{\theta}_{0g^*}}{\bar{\theta}_{1g^*} - \bar{\theta}_{1g}}$$

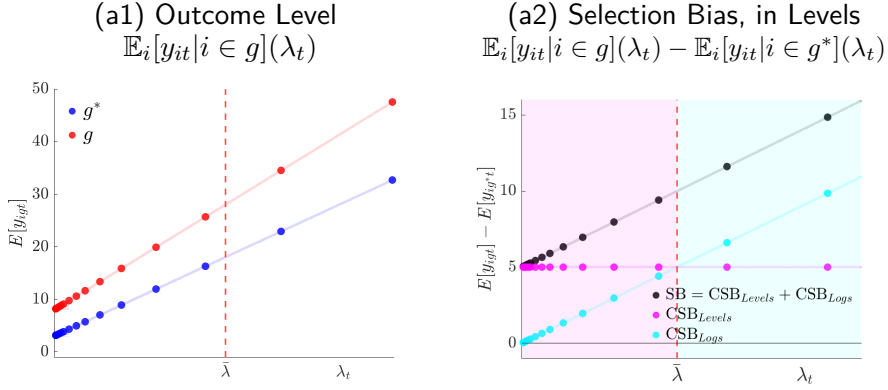
Since the numerator and the denominator share the same sign, $\bar{\lambda}_s > 0$ uniquely exists. Substituting absolute values allows this to be stated generally as $\bar{\lambda}_s = \frac{|\bar{\theta}_{0g} - \bar{\theta}_{0g^*}|}{|\bar{\theta}_{1g} - \bar{\theta}_{1g^*}|} = \bar{\lambda}$.

- When $\lambda_t < \bar{\lambda}_s$, the magnitude of the proportional wedge is strictly lower than the baseline level wedge, meaning $|(\bar{\theta}_{1g} - \bar{\theta}_{1g^*})\lambda_t| < \bar{\theta}_{0g} - \bar{\theta}_{0g^*}$. Thus $\Delta_{levels} > 0$, which mirrors the sign of the level shifter difference.
- When $\lambda_t > \bar{\lambda}_s$, the scaling factor amplifies the proportional divergence beyond the fixed additive difference, meaning $|(\bar{\theta}_{1g} - \bar{\theta}_{1g^*})\lambda_t| > \bar{\theta}_{0g} - \bar{\theta}_{0g^*}$. Thus $\Delta_{levels} < 0$, matching the sign of the proportional shifter difference.

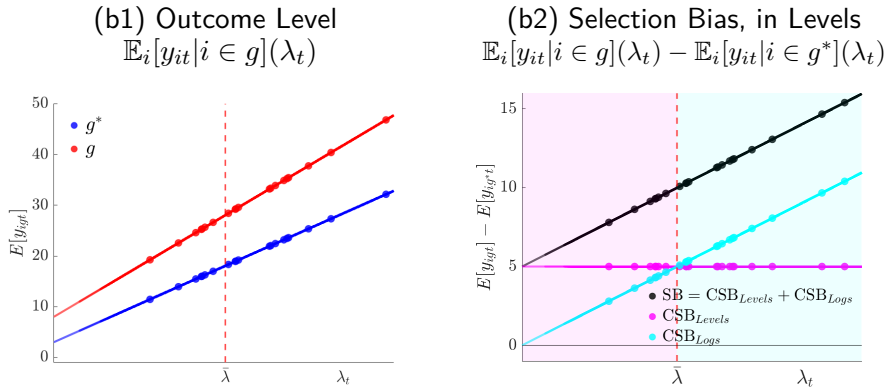
□

Figure A.1: Double Selection: Illustrating Alternative Common Time Trends λ_t

(a) A CTT with Deterministic Component: $\lambda_t = \lambda_0(1 + I_\lambda)^t$



(b) A Stochastic CTT: $\lambda_t = \bar{\lambda}e^{\eta_t}$ and $\eta_t \sim iid(0, \sigma_\eta^2)$



Notes: $\bar{\theta}_{0g} > \bar{\theta}_{0g^*}$, $\bar{\theta}_{1g} > \bar{\theta}_{1g^*} > 0$. On the figure axes, $E[y_{igt}]$ is short-hand notation for $\mathbb{E}_i[y_{it}|i \in g]$, and accordingly for group g^* and for the log outcomes.

B Appendix for Section 5

Table B.1: First-Step Normalization Coefficients by Control Group

Control group	Double Selection Estimator		Single Selection Estimator (Levels)	Single Selection Estimator (Proportional)
	ω_0	ω_1	ω_0^{single}	ω_1^{single}
Card Control	3.092 (1.426) [0.638, 5.322]	0.620 (0.250) [0.229, 1.047]	0.919 (0.053) [0.830, 1.009]	1.160 (0.010) [1.143, 1.176]
Double Selection supported:				
Alaska	8.714 (3.441) [2.277, 12.758]	-0.262 (0.437) [-0.770, 0.551]	-1.251 (0.106) [-1.427, -1.084]	0.841 (0.012) [0.822, 0.859]

continued on next page

Table B.1 – continued

Control group	Double Selection Estimator		Single Selection Estimator (Levels)	Single Selection Estimator (Proportional)
	ω_0	ω_1	ω_0^{single}	ω_1^{single}
Colorado	8.899 (4.370) [2.915, 13.456]	-0.372 (0.726) [-1.136, 0.609]	0.582 (0.103) [0.406, 0.746]	1.095 (0.019) [1.063, 1.125]
Connecticut	4.911 (0.851) [3.465, 6.270]	0.277 (0.138) [0.057, 0.510]	0.387 (0.097) [0.225, 0.550]	1.059 (0.017) [1.032, 1.086]
District of Columbia	5.952 (0.893) [4.641, 7.322]	0.112 (0.145) [-0.111, 0.320]	0.462 (0.123) [0.255, 0.664]	1.070 (0.021) [1.035, 1.106]
Georgia	4.551 (2.343) [1.371, 7.236]	0.370 (0.414) [-0.100, 0.933]	0.985 (0.088) [0.833, 1.136]	1.173 (0.018) [1.144, 1.203]
Indiana	7.157 (5.138) [1.944, 12.382]	-0.099 (0.986) [-1.111, 0.904]	1.459 (0.080) [1.318, 1.581]	1.280 (0.019) [1.247, 1.308]
Kansas	12.539 (3.957) [2.302, 14.721]	-1.066 (0.719) [-1.465, 0.790]	1.113 (0.098) [0.950, 1.267]	1.200 (0.021) [1.166, 1.233]
Louisiana	10.566 (4.015) [3.061, 13.285]	-0.744 (0.761) [-1.258, 0.673]	1.372 (0.097) [1.208, 1.522]	1.259 (0.022) [1.221, 1.294]
Maine	5.025 (0.625) [3.856, 5.913]	0.312 (0.121) [0.139, 0.536]	1.459 (0.080) [1.320, 1.584]	1.276 (0.019) [1.242, 1.306]
Maryland	3.872 (1.131) [2.056, 5.746]	0.462 (0.190) [0.144, 0.766]	0.650 (0.088) [0.495, 0.789]	1.107 (0.016) [1.079, 1.131]
Massachusetts	5.104 (0.492) [4.246, 5.854]	0.255 (0.082) [0.126, 0.399]	0.613 (0.067) [0.494, 0.714]	1.095 (0.012) [1.074, 1.113]
Mississippi	4.992 (2.051) [1.989, 8.003]	0.331 (0.411) [-0.277, 0.946]	1.648 (0.090) [1.494, 1.792]	1.328 (0.023) [1.289, 1.365]
Montana	5.007 (2.078) [1.871, 8.376]	0.302 (0.384) [-0.317, 0.883]	1.226 (0.078) [1.090, 1.348]	1.225 (0.017) [1.196, 1.251]
Nevada	4.397 (0.954) [2.980, 5.716]	0.357 (0.153) [0.145, 0.580]	0.350 (0.106) [0.178, 0.534]	1.053 (0.018) [1.023, 1.084]
New Hampshire	4.894 (0.760) [3.488, 5.970]	0.297 (0.130) [0.112, 0.537]	0.755 (0.107) [0.584, 0.945]	1.124 (0.020) [1.092, 1.158]
New Jersey	4.597 (0.779) [3.366, 5.923]	0.326 (0.124) [0.113, 0.519]	0.365 (0.069) [0.243, 0.484]	1.055 (0.011) [1.035, 1.075]
New York	4.074 (0.821) [2.798, 5.466]	0.430 (0.138) [0.193, 0.645]	0.664 (0.056) [0.569, 0.757]	1.109 (0.010) [1.093, 1.126]
North Dakota	5.950 (2.439) [1.790, 9.628]	0.133 (0.467) [-0.565, 0.931]	1.417 (0.081) [1.274, 1.542]	1.270 (0.019) [1.236, 1.299]
Oregon	4.544 (3.370) [1.016, 7.902]	0.361 (0.574) [-0.211, 0.981]	0.830 (0.116) [0.635, 1.028]	1.142 (0.023) [1.102, 1.181]
Rhode Island	4.602 (0.883) [2.948, 5.800]	0.370 (0.161) [0.155, 0.671]	1.124 (0.094) [0.977, 1.281]	1.200 (0.020) [1.167, 1.233]
South Carolina	5.163 (1.021) [3.533, 6.607]	0.280 (0.196) [0.006, 0.600]	1.351 (0.114) [1.171, 1.538]	1.251 (0.028) [1.204, 1.298]
Vermont	4.093 (1.235) [2.132, 6.076]	0.480 (0.232) [0.106, 0.859]	1.333 (0.094) [1.167, 1.476]	1.249 (0.021) [1.210, 1.282]
Virginia	5.325 (1.628) [2.995, 8.120]	0.224 (0.279) [-0.258, 0.620]	0.768 (0.089) [0.628, 0.917]	1.129 (0.017) [1.101, 1.158]

continued on next page

Table B.1 – continued

Control group	Double Selection Estimator		Single Selection Estimator (Levels)	Single Selection Estimator (Proportional)
	ω_0	ω_1	ω_0^{single}	ω_1^{single}
Washington	11.643 (3.181) [5.009, 13.671]	-0.777 (0.498) [-1.099, 0.249]	0.208 (0.119) [0.016, 0.415]	1.031 (0.020) [0.999, 1.064]
West Virginia	4.369 (1.345) [2.264, 6.499]	0.478 (0.285) [0.024, 0.929]	1.884 (0.082) [1.746, 2.013]	1.394 (0.023) [1.355, 1.431]
Wyoming	13.144 (4.045) [4.360, 15.037]	-1.234 (0.768) [-1.610, 0.432]	1.375 (0.091) [1.229, 1.533]	1.260 (0.021) [1.225, 1.296]
Double Selection not supported:				
Alabama	1.706 (2.935) [-1.605, 7.084]	1.002 (0.596) [-0.091, 1.666]	1.716 (0.079) [1.587, 1.834]	1.348 (0.020) [1.315, 1.379]
Arizona*	-17.103 (4.962) [-0.979, 13.143]	4.033 (0.843) [-1.121, 1.290]	0.755 (0.113) [0.578, 0.942]	1.128 (0.021) [1.095, 1.163]
Arkansas	5.396 (4.208) [0.140, 12.497]	0.238 (0.803) [-1.107, 1.226]	1.400 (0.092) [1.249, 1.537]	1.267 (0.021) [1.230, 1.298]
Delaware	3.365 (3.840) [0.599, 7.299]	0.593 (0.689) [-0.104, 1.082]	1.119 (0.096) [0.965, 1.288]	1.202 (0.020) [1.169, 1.237]
Florida	3.421 (1.407) [0.910, 5.470]	0.584 (0.255) [0.212, 1.046]	1.125 (0.058) [1.028, 1.224]	1.203 (0.012) [1.183, 1.223]
Hawaii	3.232 (3.066) [-0.501, 8.465]	0.590 (0.532) [-0.317, 1.261]	0.865 (0.096) [0.705, 1.017]	1.149 (0.019) [1.118, 1.179]
Idaho	3.508 (1.538) [1.506, 5.661]	0.611 (0.298) [0.190, 1.003]	1.509 (0.080) [1.388, 1.635]	1.293 (0.019) [1.263, 1.325]
Illinois	2.931 (3.816) [-1.843, 8.551]	0.633 (0.649) [-0.326, 1.444]	0.777 (0.064) [0.672, 0.875]	1.132 (0.012) [1.113, 1.150]
Iowa	2.989 (2.361) [0.487, 6.233]	0.705 (0.460) [0.079, 1.189]	1.457 (0.088) [1.319, 1.600]	1.280 (0.021) [1.247, 1.314]
Kentucky	1.904 (2.381) [-0.806, 6.021]	0.927 (0.469) [0.131, 1.445]	1.529 (0.092) [1.385, 1.683]	1.299 (0.023) [1.263, 1.336]
Michigan	2.988 (1.660) [-0.026, 5.328]	0.683 (0.311) [0.240, 1.247]	1.295 (0.061) [1.188, 1.391]	1.241 (0.013) [1.218, 1.262]
Minnesota	2.485 (2.370) [-0.126, 6.925]	0.732 (0.418) [-0.050, 1.185]	0.962 (0.091) [0.810, 1.106]	1.169 (0.018) [1.139, 1.199]
Missouri	3.068 (3.750) [-0.497, 9.978]	0.642 (0.674) [-0.612, 1.285]	1.075 (0.089) [0.937, 1.226]	1.193 (0.019) [1.164, 1.224]
Nebraska	3.300 (1.986) [0.434, 6.482]	0.680 (0.406) [0.022, 1.266]	1.728 (0.077) [1.593, 1.848]	1.351 (0.020) [1.316, 1.381]
New Mexico	4.012 (2.719) [1.373, 7.905]	0.500 (0.515) [-0.241, 1.003]	1.384 (0.098) [1.214, 1.553]	1.262 (0.023) [1.220, 1.301]
North Carolina	4.256 (4.196) [-0.985, 9.068]	0.443 (0.768) [-0.448, 1.395]	1.254 (0.093) [1.084, 1.400]	1.232 (0.020) [1.196, 1.263]
Ohio	1.473 (2.607) [-1.988, 4.593]	0.958 (0.484) [0.377, 1.603]	1.248 (0.058) [1.156, 1.350]	1.231 (0.012) [1.212, 1.252]
Oklahoma	5.659 (3.987) [0.116, 11.748]	0.180 (0.726) [-0.948, 1.182]	1.162 (0.087) [1.017, 1.316]	1.211 (0.019) [1.181, 1.243]

continued on next page

Table B.1 – continued

Control group	Double Selection Estimator		Single Selection Estimator (Levels)	Single Selection Estimator (Proportional)
	ω_0	ω_1	ω_0^{single}	ω_1^{single}
Pennsylvania	2.579 (1.723) [0.070, 5.527]	0.749 (0.316) [0.203, 1.207]	1.217 (0.072) [1.094, 1.328]	1.224 (0.015) [1.197, 1.246]
South Dakota	13.312 (7.171) [-3.271, 16.948]	-1.391 (1.500) [-2.142, 2.052]	1.849 (0.074) [1.732, 1.971]	1.385 (0.020) [1.354, 1.416]
Tennessee	2.306 (2.987) [-0.317, 8.069]	0.844 (0.582) [-0.277, 1.344]	1.502 (0.081) [1.376, 1.636]	1.292 (0.019) [1.261, 1.323]
Texas	2.148 (2.110) [-0.970, 5.315]	0.786 (0.370) [0.235, 1.340]	0.922 (0.060) [0.819, 1.015]	1.161 (0.011) [1.141, 1.179]
Utah	3.110 (2.167) [0.508, 5.924]	0.640 (0.399) [0.128, 1.116]	1.119 (0.098) [0.970, 1.281]	1.202 (0.021) [1.167, 1.238]
Wisconsin	3.030 (2.002) [-0.036, 5.606]	0.708 (0.395) [0.206, 1.325]	1.538 (0.079) [1.393, 1.663]	1.300 (0.019) [1.266, 1.331]

Notes: Bootstrap standard errors in parentheses, 90% bootstrap percentile confidence intervals in brackets, both based on 500 bootstrap draws. The two single selection estimators are restricted versions of the double selection estimator. Single selection (levels): ω_0^{single} is the intercept with the slope fixed at 1. Single selection (proportional): ω_1^{single} is the slope with no constant. *: Double Selection first-step estimates unreliable: point estimate outside bootstrap CI.

Table B.2: Event-Study Estimates: Double Selection Estimator

Control group	1 st step supports double selection	Effect relative to 1987				
		Pre-policy			Post-policy	
		1984	1985	1986	1989	1990
Card Control	0	0.003 (0.049) [-0.095, 0.050]	-0.139 (0.112) [-0.345, 0.022]	-0.186 (0.123) [-0.389, 0.007]	0.374 (0.222) [0.017, 0.731]	0.308 (0.286) [-0.192, 0.742]
<i>Pre-test passed: Constant Selection Bias supported; significant policy effect:</i>						
Idaho	0	0.017 (0.086) [-0.202, 0.068]	-0.077 (0.121) [-0.324, 0.056]	-0.150 (0.144) [-0.426, 0.043]	0.421 (0.250) [0.028, 0.728]	0.288 (0.367) [-0.310, 0.763]
Iowa	0	0.019 (0.118) [-0.324, 0.069]	-0.140 (0.152) [-0.446, 0.039]	-0.145 (0.153) [-0.430, 0.033]	0.529 (0.281) [0.186, 0.882]	0.444 (0.410) [-0.079, 0.913]
Kansas	1	-0.277 (0.152) [-0.471, 0.019]	-0.072 (0.156) [-0.472, 0.030]	-0.128 (0.153) [-0.440, 0.035]	0.930 (0.271) [0.390, 1.195]	1.626 (0.573) [0.289, 2.083]
Louisiana	1	-0.168 (0.152) [-0.473, 0.018]	-0.156 (0.157) [-0.464, 0.029]	-0.335 (0.160) [-0.506, 0.015]	1.045 (0.449) [0.279, 1.488]	1.263 (0.497) [0.393, 1.702]
Massachusetts	1	0.009 (0.046) [-0.081, 0.073]	-0.072 (0.079) [-0.208, 0.050]	-0.164 (0.108) [-0.350, 0.006]	0.386 (0.196) [0.057, 0.710]	0.468 (0.194) [0.144, 0.769]
Nevada	1	0.038 (0.070) [-0.155, 0.090]	-0.091 (0.132) [-0.346, 0.063]	-0.054 (0.113) [-0.283, 0.077]	0.519 (0.221) [0.149, 0.816]	0.487 (0.267) [0.013, 0.829]
New York	1	0.000 (0.047) [-0.107, 0.052]	-0.146 (0.112) [-0.349, 0.019]	-0.190 (0.119) [-0.392, 0.007]	0.318 (0.204) [0.008, 0.686]	0.227 (0.272) [-0.206, 0.700]
Tennessee	0	-0.026 (0.146) [-0.429, 0.047]	-0.240 (0.166) [-0.512, 0.030]	-0.135 (0.150) [-0.434, 0.044]	0.423 (0.312) [0.049, 0.884]	0.249 (0.500) [-0.308, 1.079]
Texas	0	-0.030 (0.096) [-0.228, 0.067]	-0.041 (0.107) [-0.276, 0.065]	-0.190 (0.141) [-0.423, 0.037]	0.720 (0.170) [0.409, 0.977]	0.552 (0.229) [0.177, 0.844]
Utah	0	-0.013 (0.131) [-0.338, 0.082]	-0.044 (0.139) [-0.387, 0.058]	0.032 (0.128) [-0.325, 0.089]	0.553 (0.331) [0.065, 0.902]	0.534 (0.376) [-0.091, 0.950]
Washington	1	-0.178 (0.158) [-0.474, 0.026]	-0.080 (0.144) [-0.439, 0.024]	0.009 (0.148) [-0.402, 0.053]	1.105 (0.310) [0.476, 1.392]	–
Wyoming	1	-0.050 (0.155) [-0.445, 0.053]	0.018 (0.154) [-0.442, 0.058]	0.001 (0.146) [-0.402, 0.053]	0.921 (0.257) [0.424, 1.177]	1.970 (0.662) [0.459, 2.138]

continued on next page

Table B.2 – continued

Control group	1 st step supports double selection	Effect relative to 1987				
		Pre-policy			Post-policy	
		1984	1985	1986	1989	1990
<i>Pre-test passed: Constant Selection Bias supported; no significant policy effect:</i>						
Alabama	0	-0.120 (0.155) [-0.451, 0.054]	-0.071 (0.149) [-0.434, 0.051]	0.036 (0.150) [-0.399, 0.080]	0.001 (0.521) [-0.638, 0.811]	-0.425 (0.820) [-1.427, 0.879]
Delaware	0	-0.068 (0.129) [-0.351, 0.047]	-0.107 (0.140) [-0.409, 0.034]	-0.262 (0.151) [-0.484, 0.015]	0.195 (0.773) [-0.364, 0.767]	-0.155 (1.146) [-1.019, 0.968]
Florida	0	-0.001 (0.066) [-0.154, 0.055]	-0.187 (0.131) [-0.414, 0.009]	-0.168 (0.131) [-0.420, 0.010]	0.277 (0.262) [-0.203, 0.686]	0.199 (0.336) [-0.392, 0.680]
Hawaii	0	-0.351 (0.152) [-0.496, 0.018]	-0.201 (0.151) [-0.482, 0.013]	-0.158 (0.151) [-0.449, 0.030]	-0.094 (0.760) [-1.013, 1.120]	-0.057 (0.795) [-1.175, 1.297]
Kentucky	0	0.002 (0.141) [-0.379, 0.069]	-0.007 (0.151) [-0.423, 0.069]	0.006 (0.143) [-0.384, 0.070]	0.318 (0.399) [-0.278, 0.826]	0.373 (0.379) [-0.126, 0.910]
Maryland	1	0.019 (0.085) [-0.211, 0.066]	-0.093 (0.129) [-0.371, 0.051]	-0.153 (0.141) [-0.413, 0.037]	0.354 (0.254) [-0.020, 0.705]	0.220 (0.334) [-0.320, 0.703]
Michigan	0	0.015 (0.072) [-0.159, 0.067]	-0.114 (0.132) [-0.385, 0.038]	-0.163 (0.134) [-0.417, 0.030]	0.253 (0.281) [-0.227, 0.689]	0.345 (0.299) [-0.193, 0.781]
Minnesota	0	0.024 (0.127) [-0.333, 0.071]	-0.150 (0.155) [-0.466, 0.039]	-0.105 (0.154) [-0.430, 0.058]	0.040 (0.440) [-0.592, 0.803]	-0.001 (0.589) [-0.811, 0.911]
Nebraska	0	-0.021 (0.113) [-0.312, 0.050]	-0.225 (0.151) [-0.478, 0.029]	-0.178 (0.145) [-0.417, 0.048]	0.245 (0.356) [-0.370, 0.750]	0.349 (0.349) [-0.174, 0.880]
New Hampshire	1	0.015 (0.066) [-0.141, 0.058]	-0.138 (0.127) [-0.375, 0.031]	-0.158 (0.131) [-0.404, 0.036]	0.341 (0.252) [-0.123, 0.687]	0.385 (0.291) [-0.196, 0.791]
Ohio	0	0.023 (0.089) [-0.188, 0.087]	-0.032 (0.118) [-0.299, 0.075]	-0.104 (0.136) [-0.376, 0.062]	0.228 (0.358) [-0.329, 0.653]	-0.040 (0.552) [-0.860, 0.542]
Pennsylvania	0	-0.019 (0.120) [-0.351, 0.053]	-0.037 (0.108) [-0.297, 0.060]	-0.176 (0.133) [-0.419, 0.017]	0.177 (0.287) [-0.242, 0.638]	0.033 (0.392) [-0.580, 0.713]
Rhode Island	1	-0.007 (0.087) [-0.195, 0.082]	0.000 (0.079) [-0.188, 0.075]	-0.115 (0.130) [-0.353, 0.061]	–	–
Vermont	1	0.014 (0.090) [-0.214, 0.066]	-0.119 (0.137) [-0.414, 0.049]	-0.164 (0.137) [-0.411, 0.037]	0.281 (0.292) [-0.206, 0.724]	0.100 (0.433) [-0.665, 0.745]

continued on next page

Table B.2 – continued

Control group	1 st step supports double selection	Effect relative to 1987				
		Pre-policy			Post-policy	
		1984	1985	1986	1989	1990
Wisconsin	0	-0.012 (0.114) [-0.300, 0.060]	0.007 (0.116) [-0.309, 0.062]	-0.110 (0.156) [-0.419, 0.060]	0.310 (0.347) [-0.280, 0.704]	0.206 (0.479) [-0.561, 0.802]
<i>Pre-test not passed; significant post-policy differences:</i>						
Alaska	1	-0.390 (0.131) [-0.508, -0.084]	-0.274 (0.146) [-0.499, -0.028]	-0.267 (0.139) [-0.488, -0.030]	0.728 (0.255) [0.341, 1.118]	–
Arkansas	0	-0.329 (0.143) [-0.502, -0.027]	-0.335 (0.151) [-0.501, -0.000]	-0.373 (0.144) [-0.510, -0.032]	0.536 (0.330) [0.225, 1.124]	0.626 (0.501) [0.085, 1.474]
Colorado	1	-0.386 (0.144) [-0.534, -0.038]	-0.249 (0.145) [-0.487, -0.001]	-0.256 (0.151) [-0.470, 0.005]	0.700 (0.260) [0.395, 1.120]	1.024 (0.578) [0.381, 1.734]
Maine	1	-0.029 (0.079) [-0.196, 0.066]	-0.103 (0.101) [-0.303, 0.032]	-0.226 (0.133) [-0.466, -0.015]	0.364 (0.223) [-0.026, 0.725]	0.500 (0.205) [0.108, 0.818]
Mississippi	1	-0.288 (0.134) [-0.490, -0.048]	-0.224 (0.145) [-0.467, 0.001]	-0.372 (0.132) [-0.532, -0.076]	0.138 (0.738) [-0.951, 1.137]	0.542 (0.322) [0.109, 1.087]
Montana	1	-0.219 (0.134) [-0.448, -0.010]	-0.332 (0.145) [-0.528, -0.045]	-0.357 (0.137) [-0.523, -0.098]	0.550 (0.192) [0.281, 0.888]	0.604 (0.243) [0.294, 1.010]
New Mexico	0	-0.349 (0.145) [-0.519, -0.045]	-0.158 (0.147) [-0.455, 0.009]	-0.234 (0.151) [-0.491, 0.007]	0.671 (0.188) [0.357, 0.981]	0.426 (0.403) [-0.052, 1.086]
North Dakota	1	-0.310 (0.135) [-0.512, -0.034]	-0.377 (0.127) [-0.535, -0.104]	-0.345 (0.133) [-0.511, -0.077]	0.579 (0.208) [0.297, 0.962]	0.702 (0.246) [0.417, 1.158]
Oklahoma	0	-0.307 (0.141) [-0.474, -0.020]	-0.359 (0.140) [-0.505, -0.032]	-0.364 (0.135) [-0.503, -0.063]	0.573 (0.260) [0.262, 1.012]	0.566 (0.804) [-0.385, 1.867]
South Carolina	1	-0.157 (0.131) [-0.393, 0.038]	-0.147 (0.130) [-0.394, 0.026]	-0.327 (0.149) [-0.524, -0.038]	0.506 (0.201) [0.193, 0.820]	0.659 (0.188) [0.341, 0.960]
Virginia	1	-0.238 (0.135) [-0.471, -0.019]	-0.340 (0.131) [-0.499, -0.075]	-0.362 (0.132) [-0.515, -0.098]	0.542 (0.187) [0.299, 0.902]	0.581 (0.270) [0.232, 1.029]
West Virginia	1	-0.074 (0.125) [-0.348, 0.047]	-0.290 (0.144) [-0.502, -0.034]	-0.137 (0.132) [-0.386, 0.029]	0.735 (0.168) [0.434, 0.987]	0.709 (0.162) [0.470, 0.975]

continued on next page

Table B.2 – continued

		Effect relative to 1987				
		Pre-policy			Post-policy	
Control group	1 st step supports double selection	1984	1985	1986	1989	1990
<i>Pre-test not passed; no significant post-policy differences:</i>						
Connecticut	1	-0.036 (0.085) [-0.218, 0.048]	-0.192 (0.121) [-0.407, -0.004]	-0.238 (0.138) [-0.457, -0.017]	–	–
District of Columbia	1	-0.250 (0.122) [-0.441, -0.055]	-0.334 (0.130) [-0.535, -0.097]	-0.371 (0.128) [-0.551, -0.126]	–	–
Georgia	1	-0.145 (0.129) [-0.390, 0.025]	-0.279 (0.143) [-0.499, -0.025]	-0.331 (0.140) [-0.510, -0.042]	0.384 (0.330) [-0.094, 0.809]	0.446 (0.453) [-0.158, 0.960]
Illinois	0	-0.174 (0.139) [-0.448, 0.015]	-0.262 (0.144) [-0.479, -0.006]	-0.356 (0.131) [-0.495, -0.081]	0.278 (0.426) [-0.251, 0.887]	0.112 (0.711) [-0.744, 1.128]
Indiana	1	-0.369 (0.136) [-0.519, -0.057]	-0.350 (0.135) [-0.515, -0.074]	-0.326 (0.138) [-0.492, -0.041]	0.728 (1.533) [-0.413, 2.000]	0.857 (0.863) [-0.108, 2.068]
Missouri	0	-0.172 (0.152) [-0.463, 0.018]	-0.206 (0.155) [-0.491, 0.019]	-0.352 (0.145) [-0.499, -0.024]	0.327 (0.418) [-0.037, 1.012]	0.315 (0.528) [-0.252, 1.228]
New Jersey	1	0.004 (0.047) [-0.096, 0.052]	-0.151 (0.117) [-0.364, 0.030]	-0.180 (0.120) [-0.406, -0.008]	0.264 (0.248) [-0.156, 0.665]	0.271 (0.276) [-0.177, 0.738]
North Carolina	0	-0.169 (0.126) [-0.416, -0.005]	-0.330 (0.140) [-0.527, -0.059]	-0.320 (0.136) [-0.504, -0.049]	0.375 (0.445) [-0.148, 1.051]	0.363 (0.719) [-0.401, 1.195]
Oregon	1	-0.367 (0.153) [-0.542, -0.031]	-0.197 (0.158) [-0.482, 0.025]	-0.210 (0.154) [-0.470, 0.034]	0.272 (0.597) [-0.437, 0.920]	–
South Dakota	0	-0.296 (0.152) [-0.502, -0.007]	-0.228 (0.150) [-0.504, -0.007]	-0.372 (0.162) [-0.522, -0.002]	1.635 (1.093) [-0.822, 2.220]	1.828 (1.146) [-0.623, 2.403]

Notes: Bootstrap standard errors in parentheses, 90% bootstrap percentile confidence intervals in brackets, both based on 500 bootstrap draws. “1st step supports double selection” = 1 if the 90% CIs of ω_0 and ω_1 in the double selection estimator exclude 0 and 1 respectively. Blocks: the pre-test is passed if every pre-1987 coefficient’s CI covers zero; a policy effect is significant if the 1989 or 1990 CI excludes zero. The policy event year 1988 is the year in which the minimum wage in California was raised to \$4.25. Blank post-period cells: horizon at or after the control state’s own policy change. Arizona is excluded because the first-step is unreliable, see Table B.1.

Table B.3: Event-Study Estimates: Single Selection Estimator (Levels)

Control group	1 st step supports double selection	Effect relative to 1987				
		Pre-policy			Post-policy	
		1984	1985	1986	1989	1990
Card Control	0	0.219 (0.155) [-0.036, 0.454]	-0.003 (0.163) [-0.268, 0.264]	-0.090 (0.159) [-0.358, 0.172]	0.231 (0.179) [-0.053, 0.540]	0.036 (0.174) [-0.263, 0.342]
<i>Pre-test passed: Constant Selection Bias supported; significant policy effect:</i>						
Delaware	0	0.125 (0.264) [-0.312, 0.548]	0.068 (0.262) [-0.372, 0.519]	-0.207 (0.269) [-0.620, 0.262]	-0.088 (0.289) [-0.574, 0.350]	-0.777 (0.332) [-1.341, -0.256]
Hawaii	0	-0.352 (0.283) [-0.811, 0.138]	-0.090 (0.262) [-0.511, 0.343]	-0.030 (0.253) [-0.444, 0.385]	-0.582 (0.308) [-1.063, -0.074]	-0.618 (0.312) [-1.074, -0.092]
Iowa	0	0.175 (0.238) [-0.209, 0.578]	-0.047 (0.245) [-0.431, 0.369]	-0.062 (0.259) [-0.473, 0.356]	0.496 (0.275) [0.084, 0.975]	0.315 (0.259) [-0.121, 0.716]
New Mexico	0	-0.347 (0.285) [-0.833, 0.110]	0.046 (0.252) [-0.359, 0.471]	-0.126 (0.245) [-0.523, 0.262]	0.734 (0.272) [0.259, 1.137]	0.098 (0.265) [-0.334, 0.511]
Texas	0	0.057 (0.165) [-0.204, 0.330]	0.046 (0.169) [-0.224, 0.329]	-0.149 (0.171) [-0.430, 0.145]	0.750 (0.200) [0.404, 1.060]	0.497 (0.175) [0.203, 0.773]
Vermont	1	0.410 (0.273) [-0.022, 0.858]	0.143 (0.264) [-0.296, 0.604]	0.029 (0.261) [-0.394, 0.469]	-0.073 (0.310) [-0.608, 0.462]	-0.607 (0.345) [-1.141, -0.041]
West Virginia	1	0.229 (0.238) [-0.168, 0.613]	-0.211 (0.252) [-0.612, 0.197]	0.087 (0.230) [-0.304, 0.482]	0.872 (0.254) [0.430, 1.299]	0.661 (0.244) [0.263, 1.072]
<i>Pre-test passed: Constant Selection Bias supported; no significant policy effect:</i>						
Alabama	0	-0.120 (0.236) [-0.517, 0.252]	-0.072 (0.246) [-0.459, 0.328]	0.035 (0.253) [-0.368, 0.474]	0.002 (0.280) [-0.482, 0.444]	-0.422 (0.331) [-0.979, 0.114]
Arizona	0	-0.278 (0.309) [-0.789, 0.224]	-0.296 (0.329) [-0.865, 0.250]	-0.247 (0.308) [-0.739, 0.282]	0.454 (0.325) [-0.060, 0.999]	0.135 (0.325) [-0.362, 0.686]
Florida	0	0.248 (0.173) [-0.061, 0.512]	-0.063 (0.171) [-0.351, 0.219]	-0.045 (0.173) [-0.358, 0.222]	0.040 (0.192) [-0.263, 0.349]	-0.195 (0.182) [-0.493, 0.099]
Georgia	1	0.206 (0.253) [-0.224, 0.600]	-0.138 (0.260) [-0.543, 0.325]	-0.312 (0.252) [-0.696, 0.145]	0.001 (0.284) [-0.447, 0.463]	-0.078 (0.294) [-0.513, 0.457]

continued on next page

Table B.3 – continued

Control group	1 st step supports double selection	Effect relative to 1987				
		Pre-policy			Post-policy	
		1984	1985	1986	1989	1990
Idaho	0	0.252 (0.218) [-0.115, 0.615]	0.104 (0.216) [-0.239, 0.474]	-0.028 (0.233) [-0.414, 0.363]	0.302 (0.248) [-0.066, 0.683]	-0.008 (0.254) [-0.440, 0.421]
Kentucky	0	0.030 (0.278) [-0.392, 0.500]	0.021 (0.277) [-0.412, 0.503]	0.033 (0.270) [-0.391, 0.481]	0.295 (0.318) [-0.230, 0.808]	0.343 (0.285) [-0.110, 0.817]
Michigan	0	0.184 (0.176) [-0.102, 0.443]	0.000 (0.190) [-0.340, 0.287]	-0.080 (0.171) [-0.370, 0.177]	0.088 (0.205) [-0.236, 0.417]	0.157 (0.183) [-0.158, 0.462]
Minnesota	0	0.161 (0.246) [-0.242, 0.567]	-0.073 (0.260) [-0.540, 0.335]	-0.018 (0.263) [-0.477, 0.417]	-0.169 (0.311) [-0.694, 0.368]	-0.277 (0.327) [-0.816, 0.255]
Mississippi	1	-0.162 (0.259) [-0.611, 0.250]	0.054 (0.243) [-0.327, 0.464]	-0.434 (0.268) [-0.853, 0.020]	-0.815 (1.059) [-2.837, 0.526]	0.114 (0.281) [-0.328, 0.552]
Missouri	0	-0.072 (0.251) [-0.484, 0.346]	-0.120 (0.250) [-0.527, 0.307]	-0.358 (0.255) [-0.767, 0.063]	0.171 (0.310) [-0.365, 0.702]	0.071 (0.290) [-0.382, 0.584]
Nebraska	0	0.134 (0.211) [-0.184, 0.496]	-0.160 (0.221) [-0.517, 0.216]	-0.101 (0.223) [-0.426, 0.279]	0.075 (0.262) [-0.370, 0.513]	0.159 (0.240) [-0.239, 0.551]
North Carolina	0	0.059 (0.321) [-0.545, 0.531]	-0.291 (0.191) [-0.582, 0.032]	-0.292 (0.185) [-0.603, 0.014]	0.081 (0.210) [-0.247, 0.440]	-0.127 (0.199) [-0.429, 0.209]
Ohio	0	0.040 (0.174) [-0.244, 0.307]	-0.017 (0.169) [-0.330, 0.250]	-0.093 (0.171) [-0.377, 0.187]	0.212 (0.197) [-0.121, 0.507]	-0.075 (0.185) [-0.376, 0.221]
Oregon	1	-0.395 (0.357) [-0.965, 0.167]	0.093 (0.288) [-0.357, 0.576]	0.025 (0.289) [-0.471, 0.495]	-0.325 (0.326) [-0.859, 0.207]	–
Pennsylvania	0	0.092 (0.238) [-0.333, 0.433]	0.071 (0.171) [-0.212, 0.348]	-0.120 (0.170) [-0.396, 0.154]	0.032 (0.190) [-0.257, 0.358]	-0.209 (0.178) [-0.498, 0.109]
South Carolina	1	0.343 (0.288) [-0.117, 0.802]	0.403 (0.284) [-0.059, 0.874]	-0.290 (0.376) [-0.901, 0.277]	0.243 (0.321) [-0.269, 0.788]	0.417 (0.310) [-0.093, 0.918]
Tennessee	0	0.035 (0.260) [-0.375, 0.463]	-0.217 (0.268) [-0.624, 0.241]	-0.097 (0.253) [-0.482, 0.330]	0.389 (0.293) [-0.081, 0.887]	0.156 (0.301) [-0.315, 0.679]
Utah	0	0.177 (0.314) [-0.309, 0.736]	0.135 (0.323) [-0.400, 0.639]	0.242 (0.323) [-0.280, 0.775]	0.522 (0.371) [-0.062, 1.202]	0.410 (0.346) [-0.144, 0.974]

continued on next page

Table B.3 – continued

Control group	1 st step supports double selection	Effect relative to 1987				
		Pre-policy			Post-policy	
		1984	1985	1986	1989	1990
Virginia	1	0.153 (0.241) [-0.234, 0.541]	-0.267 (0.270) [-0.694, 0.179]	-0.431 (0.262) [-0.854, 0.033]	0.311 (0.281) [-0.125, 0.781]	-0.016 (0.277) [-0.442, 0.441]
Wisconsin	0	0.128 (0.233) [-0.268, 0.470]	0.159 (0.244) [-0.275, 0.544]	-0.014 (0.258) [-0.429, 0.405]	0.187 (0.263) [-0.286, 0.580]	-0.020 (0.260) [-0.461, 0.405]
<i>Pre-test not passed; significant post-policy differences:</i>						
Massachusetts	1	1.057 (0.196) [0.745, 1.369]	0.771 (0.192) [0.477, 1.102]	0.357 (0.198) [0.058, 0.670]	-0.263 (0.229) [-0.649, 0.098]	-0.366 (0.219) [-0.729, -0.009]
Montana	1	0.087 (0.218) [-0.282, 0.435]	-0.265 (0.235) [-0.641, 0.133]	-0.392 (0.223) [-0.767, -0.040]	0.415 (0.240) [0.013, 0.835]	0.260 (0.244) [-0.102, 0.701]
New Jersey	1	0.738 (0.205) [0.406, 1.072]	0.284 (0.208) [-0.060, 0.635]	0.153 (0.198) [-0.171, 0.473]	-0.449 (0.241) [-0.885, -0.067]	-0.726 (0.221) [-1.105, -0.395]
New York	1	0.466 (0.148) [0.244, 0.719]	0.139 (0.171) [-0.142, 0.420]	0.013 (0.169) [-0.263, 0.290]	-0.068 (0.199) [-0.389, 0.269]	-0.472 (0.199) [-0.794, -0.144]
<i>Pre-test not passed; no significant post-policy differences:</i>						
Alaska	1	-0.202 (0.291) [-0.677, 0.277]	-0.695 (0.304) [-1.182, -0.190]	-0.626 (0.306) [-1.133, -0.117]	0.152 (0.294) [-0.304, 0.657]	–
Arkansas	0	-0.257 (0.246) [-0.617, 0.176]	-0.253 (0.273) [-0.672, 0.206]	-0.472 (0.246) [-0.854, -0.058]	0.304 (0.247) [-0.087, 0.734]	0.220 (0.249) [-0.161, 0.651]
Colorado	1	-0.256 (0.268) [-0.717, 0.173]	-0.663 (0.296) [-1.147, -0.189]	-0.574 (0.283) [-1.061, -0.106]	0.363 (0.317) [-0.179, 0.854]	0.027 (0.306) [-0.484, 0.522]
Connecticut	1	0.786 (0.272) [0.321, 1.212]	0.251 (0.288) [-0.223, 0.689]	0.032 (0.283) [-0.459, 0.484]	–	–
District of Columbia	1	0.551 (0.350) [0.010, 1.135]	-0.114 (0.377) [-0.720, 0.521]	-0.599 (0.376) [-1.236, 0.028]	–	–
Illinois	0	-0.072 (0.183) [-0.361, 0.225]	-0.204 (0.189) [-0.506, 0.111]	-0.364 (0.183) [-0.676, -0.066]	0.086 (0.210) [-0.237, 0.493]	-0.261 (0.187) [-0.566, 0.048]
Indiana	1	-0.166 (0.219) [-0.526, 0.187]	-0.470 (0.234) [-0.878, -0.108]	-0.502 (0.235) [-0.867, -0.105]	-0.594 (0.628) [-1.641, 0.316]	-0.293 (0.283) [-0.736, 0.194]

continued on next page

Table B.3 – continued

Control group	1 st step supports double selection	Effect relative to 1987				
		Pre-policy			Post-policy	
		1984	1985	1986	1989	1990
Kansas	1	-0.421 (0.265) [-0.828, 0.004]	-0.632 (0.273) [-1.107, -0.221]	-0.543 (0.288) [-1.049, -0.052]	0.307 (0.266) [-0.140, 0.732]	-0.066 (0.276) [-0.486, 0.377]
Louisiana	1	-0.597 (0.264) [-1.005, -0.187]	-0.636 (0.298) [-1.102, -0.155]	-0.351 (0.277) [-0.835, 0.084]	0.022 (0.308) [-0.529, 0.533]	0.068 (0.307) [-0.427, 0.598]
Maine	1	0.679 (0.219) [0.292, 1.031]	0.464 (0.235) [0.097, 0.859]	0.030 (0.241) [-0.384, 0.426]	-0.173 (0.276) [-0.645, 0.263]	-0.057 (0.272) [-0.488, 0.373]
Maryland	1	0.448 (0.230) [0.065, 0.852]	0.220 (0.271) [-0.236, 0.683]	0.067 (0.270) [-0.409, 0.519]	0.059 (0.309) [-0.484, 0.573]	-0.399 (0.304) [-0.916, 0.078]
Nevada	1	0.739 (0.303) [0.229, 1.226]	0.396 (0.340) [-0.143, 0.956]	0.464 (0.312) [-0.029, 0.995]	0.357 (0.348) [-0.252, 0.916]	0.007 (0.357) [-0.561, 0.582]
New Hampshire	1	0.882 (0.314) [0.342, 1.361]	0.389 (0.327) [-0.162, 0.910]	0.278 (0.307) [-0.240, 0.790]	-0.293 (0.334) [-0.872, 0.245]	-0.486 (0.350) [-1.070, 0.059]
North Dakota	1	-0.040 (0.208) [-0.418, 0.314]	-0.480 (0.230) [-0.898, -0.141]	-0.365 (0.214) [-0.728, -0.025]	0.388 (0.251) [-0.035, 0.774]	0.368 (0.236) [-0.011, 0.722]
Oklahoma	0	-0.107 (0.235) [-0.492, 0.288]	-0.351 (0.244) [-0.730, 0.070]	-0.465 (0.247) [-0.872, -0.068]	0.408 (0.274) [-0.047, 0.877]	-0.290 (0.338) [-0.840, 0.298]
Rhode Island	1	0.580 (0.295) [0.074, 1.050]	0.615 (0.285) [0.145, 1.072]	0.271 (0.295) [-0.243, 0.741]	–	–
South Dakota	0	-0.390 (0.201) [-0.700, -0.058]	-0.457 (0.211) [-0.789, -0.107]	-0.320 (0.190) [-0.614, -0.013]	-0.129 (0.252) [-0.548, 0.273]	-0.020 (0.214) [-0.356, 0.333]
Washington	1	-0.573 (0.299) [-1.042, -0.102]	-0.724 (0.316) [-1.248, -0.198]	-0.793 (0.371) [-1.409, -0.182]	-0.031 (0.308) [-0.555, 0.475]	–
Wyoming	1	-0.595 (0.255) [-1.030, -0.169]	-0.668 (0.276) [-1.100, -0.204]	-0.620 (0.260) [-1.071, -0.211]	0.356 (0.293) [-0.141, 0.812]	-0.233 (0.313) [-0.743, 0.268]

Notes: Bootstrap standard errors in parentheses, 90% bootstrap percentile confidence intervals in brackets, both based on 500 bootstrap draws. “1st step supports double selection” = 1 if the 90% CIs of ω_0 and ω_1 in the double selection estimator exclude 0 and 1 respectively. Blocks: the pre-test is passed if every pre-1987 coefficient’s CI covers zero; a policy effect is significant if the 1989 or 1990 CI excludes zero. The policy event year 1988 is the year in which the minimum wage in California was raised to \$4.25. Blank post-period cells: horizon at or after the control state’s own policy change.

Table B.4: Event-Study Estimates: Single Selection Estimator (Proportional)

Control group	1 st step supports double selection	Effect relative to 1987				
		Pre-policy			Post-policy	
		1984	1985	1986	1989	1990
Card Control	0	0.310 (0.165) [0.037, 0.556]	0.054 (0.173) [-0.225, 0.344]	-0.050 (0.169) [-0.331, 0.234]	0.171 (0.189) [-0.129, 0.496]	-0.079 (0.188) [-0.396, 0.256]
<i>Pre-test passed: Constant Selection Bias supported; significant policy effect:</i>						
Alabama	0	-0.040 (0.299) [-0.545, 0.422]	0.029 (0.313) [-0.464, 0.530]	0.166 (0.318) [-0.352, 0.711]	-0.208 (0.358) [-0.848, 0.354]	-0.831 (0.435) [-1.566, -0.145]
Delaware	0	0.221 (0.307) [-0.298, 0.713]	0.155 (0.305) [-0.362, 0.674]	-0.180 (0.314) [-0.666, 0.364]	-0.228 (0.342) [-0.835, 0.293]	-1.086 (0.403) [-1.817, -0.478]
Hawaii	0	-0.352 (0.317) [-0.871, 0.188]	-0.049 (0.292) [-0.516, 0.436]	0.017 (0.283) [-0.461, 0.476]	-0.760 (0.351) [-1.300, -0.192]	-0.823 (0.358) [-1.353, -0.217]
New Mexico	0	-0.346 (0.350) [-0.926, 0.215]	0.152 (0.306) [-0.335, 0.680]	-0.070 (0.293) [-0.543, 0.403]	0.767 (0.322) [0.191, 1.238]	-0.073 (0.326) [-0.608, 0.436]
Pennsylvania	0	0.191 (0.280) [-0.304, 0.583]	0.168 (0.192) [-0.149, 0.471]	-0.070 (0.191) [-0.379, 0.239]	-0.097 (0.214) [-0.431, 0.274]	-0.424 (0.204) [-0.760, -0.067]
Texas	0	0.123 (0.179) [-0.160, 0.413]	0.111 (0.184) [-0.191, 0.420]	-0.118 (0.187) [-0.424, 0.205]	0.773 (0.216) [0.397, 1.114]	0.456 (0.194) [0.136, 0.760]
West Virginia	1	0.458 (0.309) [-0.058, 0.956]	-0.152 (0.333) [-0.679, 0.392]	0.255 (0.298) [-0.250, 0.773]	0.976 (0.327) [0.395, 1.518]	0.624 (0.317) [0.091, 1.132]
<i>Pre-test passed: Constant Selection Bias supported; no significant policy effect:</i>						
Arizona	0	-0.268 (0.342) [-0.839, 0.303]	-0.287 (0.365) [-0.917, 0.305]	-0.235 (0.343) [-0.794, 0.358]	0.434 (0.360) [-0.151, 1.024]	0.056 (0.366) [-0.504, 0.671]
Arkansas	0	-0.232 (0.298) [-0.662, 0.279]	-0.224 (0.330) [-0.716, 0.325]	-0.507 (0.297) [-0.990, 0.006]	0.223 (0.296) [-0.239, 0.723]	0.078 (0.301) [-0.404, 0.596]
Georgia	1	0.302 (0.286) [-0.187, 0.745]	-0.099 (0.295) [-0.557, 0.439]	-0.307 (0.287) [-0.739, 0.217]	-0.104 (0.324) [-0.624, 0.418]	-0.221 (0.342) [-0.738, 0.381]
Idaho	0	0.429 (0.265) [-0.018, 0.873]	0.240 (0.263) [-0.188, 0.695]	0.064 (0.283) [-0.398, 0.545]	0.212 (0.302) [-0.233, 0.688]	-0.231 (0.317) [-0.749, 0.300]

continued on next page

Table B.4 – continued

Control group	1 st step supports double selection	Effect relative to 1987				
		Pre-policy			Post-policy	
		1984	1985	1986	1989	1990
Iowa	0	0.322 (0.289) [-0.146, 0.806]	0.041 (0.301) [-0.427, 0.553]	0.017 (0.315) [-0.493, 0.513]	0.464 (0.331) [-0.015, 1.046]	0.192 (0.320) [-0.343, 0.697]
Kentucky	0	0.144 (0.346) [-0.363, 0.727]	0.135 (0.342) [-0.390, 0.726]	0.145 (0.333) [-0.398, 0.679]	0.201 (0.395) [-0.453, 0.841]	0.221 (0.360) [-0.364, 0.828]
Michigan	0	0.313 (0.199) [-0.011, 0.611]	0.087 (0.216) [-0.302, 0.417]	-0.017 (0.191) [-0.353, 0.270]	-0.038 (0.230) [-0.409, 0.325]	0.013 (0.209) [-0.356, 0.350]
Minnesota	0	0.248 (0.277) [-0.217, 0.722]	-0.025 (0.295) [-0.543, 0.445]	0.037 (0.299) [-0.488, 0.520]	-0.300 (0.358) [-0.912, 0.313]	-0.451 (0.380) [-1.058, 0.171]
Mississippi	1	-0.100 (0.327) [-0.659, 0.432]	0.191 (0.299) [-0.286, 0.692]	-0.464 (0.339) [-1.012, 0.094]	-1.282 (1.398) [-3.881, 0.486]	-0.096 (0.361) [-0.669, 0.452]
Missouri	0	-0.019 (0.288) [-0.489, 0.467]	-0.073 (0.286) [-0.544, 0.411]	-0.361 (0.293) [-0.827, 0.136]	0.086 (0.360) [-0.540, 0.684]	-0.060 (0.339) [-0.603, 0.538]
Montana	1	0.185 (0.254) [-0.241, 0.586]	-0.243 (0.274) [-0.694, 0.226]	-0.403 (0.260) [-0.828, 0.023]	0.372 (0.278) [-0.090, 0.840]	0.149 (0.290) [-0.274, 0.653]
Nebraska	0	0.305 (0.263) [-0.103, 0.756]	-0.090 (0.277) [-0.546, 0.399]	-0.016 (0.280) [-0.443, 0.455]	-0.113 (0.331) [-0.674, 0.434]	-0.049 (0.305) [-0.575, 0.458]
North Carolina	0	0.154 (0.387) [-0.554, 0.729]	-0.275 (0.218) [-0.608, 0.098]	-0.280 (0.210) [-0.633, 0.068]	-0.041 (0.234) [-0.408, 0.358]	-0.332 (0.230) [-0.679, 0.060]
Ohio	0	0.130 (0.196) [-0.200, 0.430]	0.062 (0.189) [-0.291, 0.372]	-0.036 (0.192) [-0.353, 0.282]	0.120 (0.222) [-0.255, 0.455]	-0.266 (0.212) [-0.618, 0.071]
Oregon	1	-0.401 (0.401) [-1.042, 0.251]	0.157 (0.319) [-0.347, 0.681]	0.077 (0.322) [-0.470, 0.609]	-0.457 (0.368) [-1.064, 0.157]	–
Tennessee	0	0.147 (0.320) [-0.347, 0.678]	-0.175 (0.327) [-0.672, 0.391]	-0.025 (0.312) [-0.511, 0.490]	0.325 (0.362) [-0.268, 0.934]	-0.019 (0.378) [-0.613, 0.618]
Utah	0	0.284 (0.361) [-0.282, 0.916]	0.235 (0.375) [-0.397, 0.802]	0.360 (0.370) [-0.253, 0.974]	0.504 (0.433) [-0.188, 1.283]	0.341 (0.408) [-0.331, 1.002]
Virginia	1	0.218 (0.265) [-0.199, 0.638]	-0.255 (0.298) [-0.729, 0.240]	-0.443 (0.288) [-0.908, 0.073]	0.272 (0.310) [-0.220, 0.796]	-0.115 (0.310) [-0.595, 0.406]

continued on next page

Table B.4 – continued

Control group	1 st step supports double selection	Effect relative to 1987				
		Pre-policy			Post-policy	
		1984	1985	1986	1989	1990
Wisconsin	0	0.271 (0.286) [-0.219, 0.699]	0.316 (0.298) [-0.234, 0.773]	0.084 (0.318) [-0.415, 0.609]	0.060 (0.327) [-0.525, 0.553]	-0.252 (0.327) [-0.783, 0.294]
<i>Pre-test not passed; significant post-policy differences:</i>						
Florida	0	0.370 (0.192) [0.027, 0.666]	-0.002 (0.189) [-0.320, 0.302]	0.016 (0.190) [-0.320, 0.312]	-0.075 (0.213) [-0.416, 0.268]	-0.388 (0.206) [-0.728, -0.058]
Illinois	0	-0.035 (0.198) [-0.338, 0.292]	-0.183 (0.203) [-0.514, 0.152]	-0.366 (0.197) [-0.707, -0.044]	0.017 (0.226) [-0.336, 0.456]	-0.395 (0.206) [-0.728, -0.060]
Massachusetts	1	1.191 (0.205) [0.869, 1.513]	0.878 (0.200) [0.568, 1.228]	0.423 (0.209) [0.109, 0.751]	-0.346 (0.247) [-0.761, 0.047]	-0.472 (0.240) [-0.870, -0.078]
New Hampshire	1	1.034 (0.342) [0.436, 1.573]	0.482 (0.362) [-0.130, 1.055]	0.354 (0.335) [-0.211, 0.909]	-0.405 (0.376) [-1.064, 0.197]	-0.639 (0.397) [-1.322, -0.039]
New Jersey	1	0.798 (0.211) [0.462, 1.144]	0.320 (0.215) [-0.031, 0.684]	0.181 (0.204) [-0.158, 0.510]	-0.508 (0.253) [-0.967, -0.111]	-0.808 (0.236) [-1.219, -0.455]
New York	1	0.555 (0.156) [0.323, 0.823]	0.194 (0.180) [-0.104, 0.485]	0.052 (0.178) [-0.235, 0.342]	-0.142 (0.212) [-0.489, 0.215]	-0.606 (0.216) [-0.949, -0.253]
Vermont	1	0.599 (0.324) [0.072, 1.152]	0.269 (0.313) [-0.288, 0.811]	0.121 (0.311) [-0.391, 0.638]	-0.243 (0.379) [-0.896, 0.418]	-0.945 (0.431) [-1.631, -0.257]
<i>Pre-test not passed; no significant post-policy differences:</i>						
Alaska	1	-0.225 (0.254) [-0.638, 0.182]	-0.642 (0.262) [-1.073, -0.204]	-0.581 (0.265) [-1.017, -0.135]	0.225 (0.261) [-0.174, 0.673]	–
Colorado	1	-0.247 (0.288) [-0.744, 0.220]	-0.692 (0.318) [-1.202, -0.172]	-0.596 (0.305) [-1.116, -0.090]	0.340 (0.342) [-0.251, 0.864]	-0.042 (0.334) [-0.600, 0.497]
Connecticut	1	0.853 (0.280) [0.375, 1.290]	0.287 (0.301) [-0.216, 0.749]	0.054 (0.297) [-0.467, 0.527]	–	–
District of Columbia	1	0.614 (0.370) [0.039, 1.229]	-0.097 (0.399) [-0.734, 0.570]	-0.617 (0.399) [-1.278, 0.039]	–	–
Indiana	1	-0.114 (0.264) [-0.555, 0.319]	-0.500 (0.283) [-1.009, -0.070]	-0.547 (0.285) [-0.992, -0.077]	-0.931 (0.803) [-2.297, 0.208]	-0.587 (0.353) [-1.149, 0.023]

continued on next page

Table B.4 – continued

Control group	1 st step supports double selection	Effect relative to 1987				
		Pre-policy			Post-policy	
		1984	1985	1986	1989	1990
Kansas	1	-0.435 (0.307) [-0.913, 0.063]	-0.687 (0.316) [-1.228, -0.214]	-0.583 (0.334) [-1.159, -0.029]	0.247 (0.305) [-0.279, 0.737]	-0.230 (0.324) [-0.736, 0.286]
Louisiana	1	-0.661 (0.318) [-1.159, -0.161]	-0.708 (0.360) [-1.280, -0.134]	-0.354 (0.336) [-0.949, 0.175]	-0.130 (0.377) [-0.782, 0.510]	-0.109 (0.377) [-0.707, 0.551]
Maine	1	0.962 (0.257) [0.519, 1.360]	0.692 (0.282) [0.252, 1.156]	0.133 (0.290) [-0.363, 0.597]	-0.389 (0.337) [-0.979, 0.128]	-0.281 (0.340) [-0.837, 0.239]
Maryland	1	0.534 (0.249) [0.117, 0.964]	0.282 (0.295) [-0.223, 0.782]	0.111 (0.294) [-0.403, 0.590]	-0.000 (0.339) [-0.602, 0.557]	-0.523 (0.340) [-1.095, 0.006]
Nevada	1	0.797 (0.311) [0.274, 1.293]	0.436 (0.353) [-0.125, 1.010]	0.506 (0.322) [-0.009, 1.056]	0.344 (0.366) [-0.290, 0.926]	-0.032 (0.379) [-0.649, 0.571]
North Dakota	1	0.044 (0.249) [-0.408, 0.465]	-0.512 (0.276) [-1.010, -0.095]	-0.371 (0.255) [-0.795, 0.032]	0.329 (0.296) [-0.171, 0.783]	0.264 (0.281) [-0.204, 0.689]
Oklahoma	0	-0.055 (0.273) [-0.503, 0.401]	-0.349 (0.283) [-0.805, 0.134]	-0.491 (0.289) [-0.973, -0.028]	0.366 (0.318) [-0.182, 0.896]	-0.511 (0.405) [-1.172, 0.179]
Rhode Island	1	0.765 (0.341) [0.174, 1.317]	0.810 (0.325) [0.265, 1.334]	0.393 (0.340) [-0.205, 0.930]	–	–
South Carolina	1	0.518 (0.344) [-0.037, 1.055]	0.595 (0.339) [0.023, 1.151]	-0.277 (0.458) [-1.024, 0.412]	0.151 (0.390) [-0.463, 0.821]	0.333 (0.378) [-0.291, 0.931]
South Dakota	0	-0.406 (0.253) [-0.795, 0.012]	-0.494 (0.267) [-0.889, -0.045]	-0.312 (0.234) [-0.687, 0.091]	-0.413 (0.318) [-0.953, 0.086]	-0.318 (0.274) [-0.745, 0.139]
Washington	1	-0.580 (0.307) [-1.058, -0.096]	-0.735 (0.322) [-1.262, -0.194]	-0.807 (0.377) [-1.423, -0.180]	-0.051 (0.319) [-0.595, 0.472]	–
Wyoming	1	-0.659 (0.310) [-1.180, -0.139]	-0.748 (0.335) [-1.272, -0.182]	-0.692 (0.315) [-1.242, -0.197]	0.290 (0.353) [-0.317, 0.824]	-0.490 (0.389) [-1.164, 0.099]

Notes: Bootstrap standard errors in parentheses, 90% bootstrap percentile confidence intervals in brackets, both based on 500 bootstrap draws. “1st step supports double selection” = 1 if the 90% CIs of ω_0 and ω_1 in the double selection estimator exclude 0 and 1 respectively. Blocks: the pre-test is passed if every pre-1987 coefficient’s CI covers zero; a policy effect is significant if the 1989 or 1990 CI excludes zero. The policy event year 1988 is the year in which the minimum wage in California was raised to \$4.25. Blank post-period cells: horizon at or after the control state’s own policy change.

Table B.5: Event-Study Estimates: DiD Log Wages

Control group	1 st step supports double selection	Effect relative to 1987				
		Pre-policy			Post-policy	
		1984	1985	1986	1989	1990
Card Control	0	0.047 (0.021) [0.014, 0.080]	0.004 (0.021) [-0.030, 0.039]	-0.010 (0.021) [-0.046, 0.025]	0.037 (0.022) [0.003, 0.073]	0.009 (0.020) [-0.023, 0.046]
<i>Pre-test passed: Constant Selection Bias supported; significant policy effect:</i>						
Delaware	0	0.021 (0.039) [-0.048, 0.084]	0.008 (0.040) [-0.059, 0.079]	-0.038 (0.042) [-0.105, 0.032]	-0.051 (0.040) [-0.120, 0.011]	-0.134 (0.042) [-0.210, -0.066]
Hawaii	0	-0.040 (0.037) [-0.098, 0.026]	-0.002 (0.036) [-0.054, 0.060]	0.011 (0.034) [-0.045, 0.071]	-0.083 (0.038) [-0.145, -0.022]	-0.097 (0.036) [-0.151, -0.035]
Iowa	0	0.033 (0.035) [-0.027, 0.093]	0.015 (0.036) [-0.042, 0.072]	-0.006 (0.038) [-0.072, 0.056]	0.064 (0.037) [0.012, 0.128]	0.022 (0.036) [-0.039, 0.078]
Montana	1	0.023 (0.032) [-0.032, 0.072]	-0.022 (0.033) [-0.075, 0.034]	-0.049 (0.031) [-0.100, 0.002]	0.056 (0.032) [0.000, 0.108]	0.032 (0.031) [-0.017, 0.089]
New Mexico	0	-0.017 (0.041) [-0.086, 0.049]	0.022 (0.039) [-0.038, 0.090]	-0.015 (0.036) [-0.074, 0.041]	0.100 (0.039) [0.030, 0.159]	-0.007 (0.038) [-0.069, 0.053]
Oklahoma	0	0.001 (0.035) [-0.056, 0.063]	-0.053 (0.035) [-0.111, 0.006]	-0.043 (0.035) [-0.102, 0.012]	0.069 (0.038) [0.004, 0.129]	0.005 (0.037) [-0.051, 0.070]
Texas	0	0.022 (0.023) [-0.015, 0.059]	0.010 (0.024) [-0.030, 0.049]	-0.008 (0.024) [-0.046, 0.036]	0.127 (0.025) [0.084, 0.168]	0.080 (0.023) [0.042, 0.115]
West Virginia	1	0.034 (0.037) [-0.031, 0.094]	-0.039 (0.037) [-0.100, 0.018]	0.012 (0.035) [-0.046, 0.071]	0.121 (0.037) [0.059, 0.184]	0.068 (0.034) [0.015, 0.125]
<i>Pre-test passed: Constant Selection Bias supported; no significant policy effect:</i>						
Alabama	0	-0.019 (0.037) [-0.081, 0.039]	-0.007 (0.039) [-0.066, 0.058]	0.002 (0.039) [-0.058, 0.073]	-0.008 (0.037) [-0.072, 0.054]	-0.054 (0.042) [-0.123, 0.012]
Arizona	0	-0.043 (0.043) [-0.111, 0.029]	-0.066 (0.047) [-0.146, 0.009]	-0.036 (0.045) [-0.108, 0.037]	0.039 (0.045) [-0.035, 0.116]	-0.009 (0.045) [-0.076, 0.066]
Arkansas	0	-0.008 (0.038) [-0.059, 0.057]	-0.014 (0.040) [-0.073, 0.055]	-0.050 (0.036) [-0.104, 0.013]	0.042 (0.036) [-0.016, 0.105]	0.032 (0.035) [-0.020, 0.095]

continued on next page

Table B.5 – continued

Control group	1 st step supports double selection	Effect relative to 1987				
		Pre-policy			Post-policy	
		1984	1985	1986	1989	1990
Georgia	1	0.059 (0.037) [-0.001, 0.116]	-0.002 (0.037) [-0.061, 0.065]	-0.036 (0.036) [-0.092, 0.026]	0.010 (0.038) [-0.050, 0.076]	-0.010 (0.039) [-0.068, 0.055]
Kentucky	0	0.006 (0.042) [-0.061, 0.074]	0.002 (0.040) [-0.062, 0.069]	0.005 (0.039) [-0.067, 0.067]	0.049 (0.042) [-0.019, 0.116]	0.043 (0.039) [-0.018, 0.106]
Minnesota	0	0.028 (0.034) [-0.031, 0.082]	-0.017 (0.036) [-0.081, 0.038]	-0.010 (0.036) [-0.071, 0.045]	-0.037 (0.038) [-0.097, 0.030]	-0.037 (0.038) [-0.106, 0.021]
Mississippi	1	-0.006 (0.039) [-0.074, 0.054]	0.018 (0.037) [-0.046, 0.083]	-0.055 (0.040) [-0.121, 0.010]	0.031 (0.041) [-0.043, 0.093]	0.032 (0.037) [-0.027, 0.087]
Missouri	0	-0.001 (0.035) [-0.058, 0.059]	-0.021 (0.036) [-0.078, 0.040]	-0.051 (0.035) [-0.109, 0.009]	0.043 (0.040) [-0.025, 0.109]	0.006 (0.038) [-0.056, 0.074]
Nebraska	0	0.027 (0.033) [-0.024, 0.084]	-0.030 (0.032) [-0.084, 0.024]	-0.015 (0.032) [-0.060, 0.041]	0.004 (0.035) [-0.054, 0.064]	-0.011 (0.033) [-0.061, 0.043]
Ohio	0	0.016 (0.025) [-0.025, 0.054]	0.002 (0.024) [-0.042, 0.039]	-0.006 (0.024) [-0.046, 0.034]	0.030 (0.025) [-0.011, 0.071]	-0.010 (0.024) [-0.050, 0.028]
Oregon	1	-0.043 (0.040) [-0.111, 0.028]	-0.002 (0.040) [-0.064, 0.063]	-0.003 (0.039) [-0.070, 0.064]	-0.059 (0.042) [-0.126, 0.011]	–
South Carolina	1	0.032 (0.038) [-0.030, 0.097]	0.044 (0.037) [-0.017, 0.110]	-0.019 (0.039) [-0.083, 0.043]	0.022 (0.039) [-0.039, 0.091]	0.030 (0.038) [-0.037, 0.090]
South Dakota	0	-0.038 (0.031) [-0.087, 0.013]	-0.045 (0.031) [-0.092, 0.009]	-0.036 (0.030) [-0.086, 0.014]	-0.014 (0.034) [-0.074, 0.040]	-0.021 (0.031) [-0.068, 0.033]
Tennessee	0	0.004 (0.039) [-0.058, 0.073]	-0.044 (0.040) [-0.108, 0.028]	-0.016 (0.039) [-0.078, 0.050]	0.050 (0.040) [-0.017, 0.121]	0.006 (0.041) [-0.057, 0.076]
Utah	0	0.006 (0.038) [-0.054, 0.069]	-0.002 (0.039) [-0.061, 0.063]	0.011 (0.040) [-0.054, 0.083]	0.052 (0.043) [-0.013, 0.125]	0.019 (0.039) [-0.047, 0.083]
Wisconsin	0	0.022 (0.033) [-0.033, 0.073]	0.028 (0.034) [-0.035, 0.084]	-0.002 (0.036) [-0.058, 0.059]	0.010 (0.035) [-0.050, 0.066]	-0.034 (0.035) [-0.089, 0.026]
Pre-test not passed; significant post-policy differences:						
Idaho	0	0.062 (0.034) [0.003, 0.119]	0.036 (0.033) [-0.018, 0.094]	0.012 (0.034) [-0.044, 0.070]	0.060 (0.035) [0.008, 0.117]	0.006 (0.034) [-0.051, 0.064]

continued on next page

Table B.5 – continued

Control group	1 st step supports double selection	Effect relative to 1987				
		Pre-policy			Post-policy	
		1984	1985	1986	1989	1990
Massachusetts	1	0.169 (0.027) [0.125, 0.210]	0.116 (0.026) [0.073, 0.159]	0.050 (0.026) [0.009, 0.091]	-0.031 (0.027) [-0.076, 0.011]	-0.049 (0.026) [-0.091, -0.005]
New Jersey	1	0.102 (0.028) [0.056, 0.148]	0.036 (0.027) [-0.009, 0.081]	0.006 (0.026) [-0.038, 0.047]	-0.047 (0.029) [-0.097, -0.000]	-0.093 (0.026) [-0.140, -0.055]
New York	1	0.079 (0.020) [0.049, 0.115]	0.025 (0.022) [-0.011, 0.060]	0.007 (0.022) [-0.028, 0.044]	0.001 (0.023) [-0.037, 0.040]	-0.042 (0.021) [-0.073, -0.008]
Vermont	1	0.079 (0.041) [0.012, 0.152]	0.019 (0.041) [-0.049, 0.085]	0.002 (0.039) [-0.063, 0.067]	-0.019 (0.043) [-0.088, 0.056]	-0.099 (0.043) [-0.169, -0.025]
<i>Pre-test not passed; no significant post-policy differences:</i>						
Alaska	1	-0.039 (0.036) [-0.101, 0.018]	-0.089 (0.034) [-0.144, -0.032]	-0.082 (0.036) [-0.140, -0.022]	0.033 (0.034) [-0.021, 0.090]	–
Colorado	1	-0.018 (0.038) [-0.085, 0.045]	-0.073 (0.039) [-0.139, -0.010]	-0.078 (0.039) [-0.143, -0.012]	0.060 (0.041) [-0.011, 0.126]	0.015 (0.041) [-0.050, 0.082]
Connecticut	1	0.132 (0.038) [0.067, 0.195]	0.046 (0.039) [-0.021, 0.107]	0.030 (0.039) [-0.039, 0.089]	–	–
District of Columbia	1	0.085 (0.053) [0.002, 0.174]	-0.013 (0.054) [-0.103, 0.073]	-0.087 (0.052) [-0.170, 0.004]	–	–
Florida	0	0.051 (0.024) [0.008, 0.089]	-0.003 (0.023) [-0.040, 0.032]	-0.003 (0.024) [-0.049, 0.034]	0.005 (0.023) [-0.036, 0.043]	-0.017 (0.022) [-0.056, 0.019]
Illinois	0	0.000 (0.026) [-0.041, 0.046]	-0.017 (0.026) [-0.063, 0.027]	-0.053 (0.025) [-0.094, -0.012]	0.018 (0.026) [-0.023, 0.065]	-0.030 (0.024) [-0.068, 0.010]
Indiana	1	0.001 (0.033) [-0.052, 0.055]	-0.063 (0.033) [-0.121, -0.013]	-0.056 (0.033) [-0.109, 0.002]	-0.010 (0.039) [-0.075, 0.054]	-0.052 (0.038) [-0.114, 0.013]
Kansas	1	-0.065 (0.038) [-0.125, -0.004]	-0.112 (0.040) [-0.178, -0.051]	-0.089 (0.039) [-0.158, -0.026]	0.019 (0.037) [-0.045, 0.083]	-0.031 (0.038) [-0.095, 0.028]
Louisiana	1	-0.087 (0.040) [-0.152, -0.023]	-0.087 (0.043) [-0.153, -0.016]	-0.046 (0.043) [-0.121, 0.021]	0.002 (0.043) [-0.072, 0.075]	-0.013 (0.040) [-0.077, 0.059]
Maine	1	0.127 (0.033) [0.067, 0.179]	0.087 (0.035) [0.035, 0.143]	0.004 (0.036) [-0.060, 0.061]	-0.048 (0.038) [-0.114, 0.012]	-0.032 (0.037) [-0.097, 0.026]

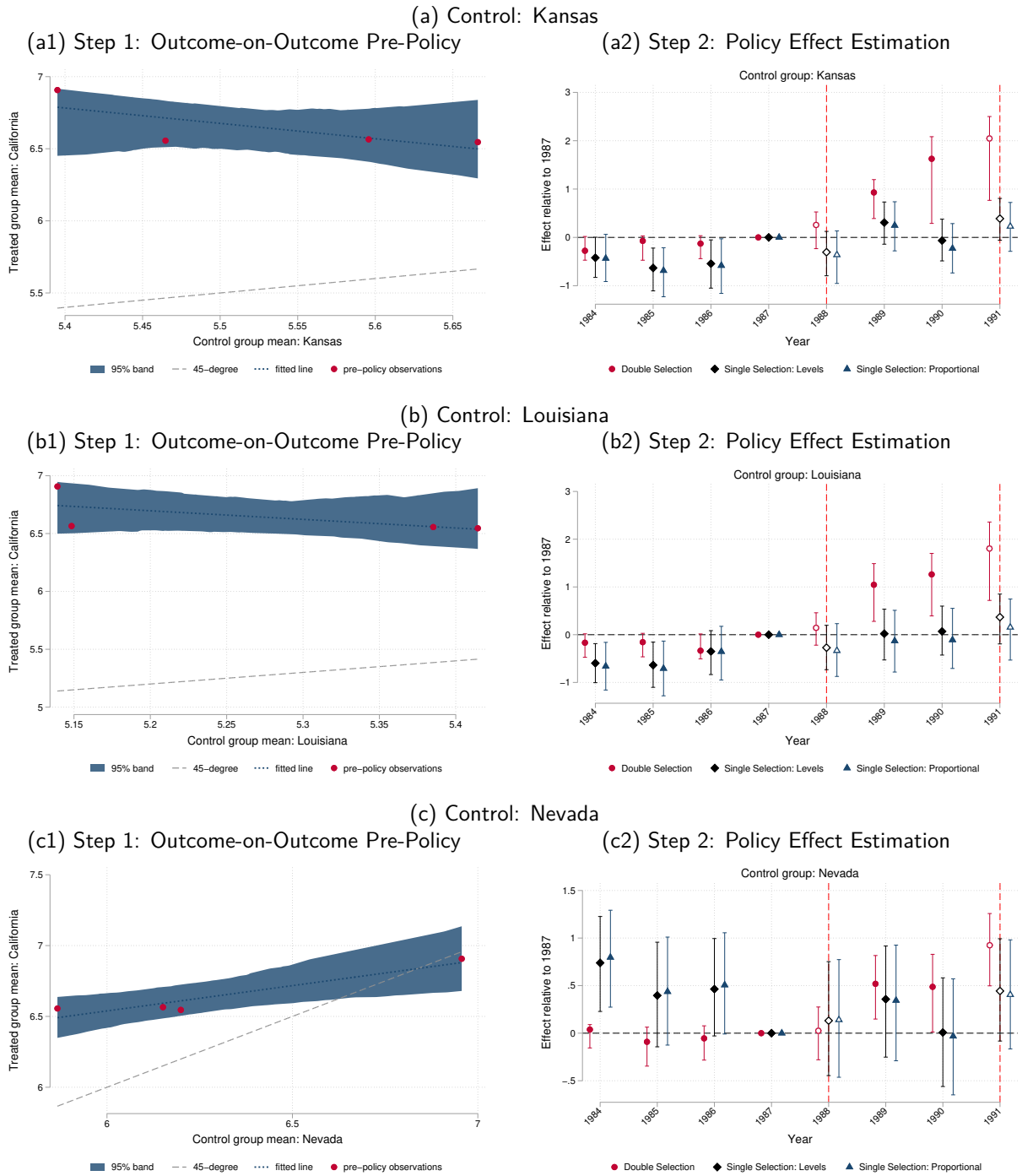
continued on next page

Table B.5 – continued

Control group	1 st step supports double selection	Effect relative to 1987				
		Pre-policy			Post-policy	
		1984	1985	1986	1989	1990
Maryland	1	0.093 (0.031) [0.044, 0.147]	0.044 (0.037) [-0.017, 0.103]	0.026 (0.036) [-0.034, 0.087]	0.017 (0.039) [-0.049, 0.083]	-0.050 (0.039) [-0.119, 0.008]
Michigan	0	0.043 (0.024) [0.002, 0.081]	0.021 (0.025) [-0.025, 0.056]	-0.004 (0.023) [-0.045, 0.030]	0.026 (0.025) [-0.015, 0.065]	0.012 (0.022) [-0.027, 0.049]
Nevada	1	0.095 (0.041) [0.029, 0.164]	0.055 (0.043) [-0.013, 0.125]	0.054 (0.042) [-0.015, 0.129]	0.049 (0.041) [-0.023, 0.115]	0.012 (0.041) [-0.059, 0.078]
New Hampshire	1	0.166 (0.046) [0.088, 0.238]	0.093 (0.044) [0.016, 0.160]	0.045 (0.044) [-0.027, 0.123]	-0.031 (0.044) [-0.106, 0.037]	-0.055 (0.043) [-0.128, 0.011]
North Carolina	0	0.059 (0.028) [0.014, 0.105]	-0.033 (0.025) [-0.072, 0.010]	-0.037 (0.025) [-0.080, 0.006]	0.006 (0.026) [-0.033, 0.049]	-0.037 (0.025) [-0.073, 0.009]
North Dakota	1	0.003 (0.031) [-0.049, 0.055]	-0.065 (0.032) [-0.120, -0.015]	-0.065 (0.031) [-0.118, -0.014]	0.047 (0.033) [-0.011, 0.098]	0.030 (0.031) [-0.018, 0.080]
Pennsylvania	0	0.042 (0.024) [0.002, 0.085]	0.016 (0.025) [-0.024, 0.056]	-0.009 (0.024) [-0.049, 0.030]	-0.006 (0.025) [-0.046, 0.035]	-0.035 (0.023) [-0.073, 0.003]
Rhode Island	1	0.106 (0.044) [0.032, 0.176]	0.112 (0.042) [0.042, 0.179]	0.042 (0.043) [-0.030, 0.110]	–	–
Virginia	1	0.021 (0.033) [-0.034, 0.076]	-0.034 (0.036) [-0.083, 0.029]	-0.068 (0.035) [-0.119, -0.003]	0.032 (0.036) [-0.030, 0.092]	-0.016 (0.035) [-0.072, 0.044]
Washington	1	-0.083 (0.040) [-0.147, -0.017]	-0.098 (0.039) [-0.168, -0.035]	-0.096 (0.043) [-0.168, -0.028]	-0.014 (0.039) [-0.079, 0.047]	–
Wyoming	1	-0.106 (0.041) [-0.176, -0.038]	-0.106 (0.043) [-0.174, -0.031]	-0.098 (0.041) [-0.170, -0.037]	0.041 (0.044) [-0.036, 0.110]	-0.049 (0.042) [-0.122, 0.018]

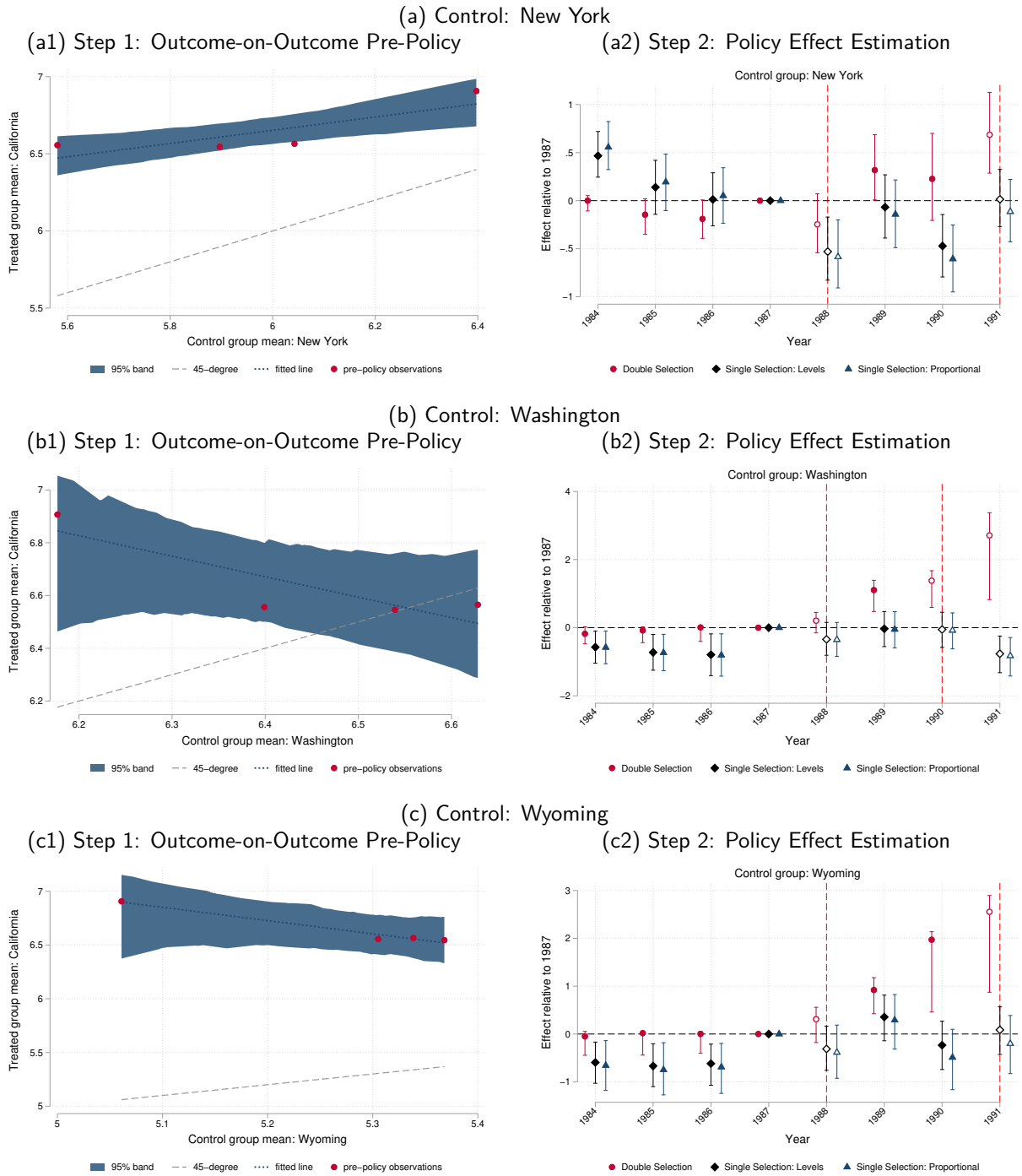
Notes: Bootstrap standard errors in parentheses, 90% bootstrap percentile confidence intervals in brackets, both based on 500 bootstrap draws. “1st step supports double selection” = 1 if the 90% CIs of ω_0 and ω_1 in the double selection estimator exclude 0 and 1 respectively. Blocks: the pre-test is passed if every pre-1987 coefficient’s CI covers zero; a policy effect is significant if the 1989 or 1990 CI excludes zero. The policy event year 1988 is the year in which the minimum wage in California was raised to \$4.25. Blank post-period cells: horizon at or after the control state’s own policy change. Coefficients are in log differences, not dollars.

Figure B.1: Causal Inference with Double Selection: Additional States 1/2



Notes: Within each panel (a)–(c), the first figure shows the first-step regression and data points; the second shows second-step event study regressions for the resulting normalized wage measures under double selection (red circles), single selection in levels (black diamonds) and proportional single selection (blue triangles). The first vertical red dashed line indicates the minimum wage increase in California; the second indicates the federal minimum wage increase or the increase of the state level minimum wage (whichever comes first). Hollow markers indicate years in which workers in the control group are subject to the higher minimum wage. Whiskers in the event study plots show 5th and 95th percentiles of 500 bootstraps.

Figure B.2: Causal Inference with Double Selection: Additional States 2/2



Notes: Within each panel (a)–(c), the first figure shows the first-step regression and data points; the second shows second-step event study regressions for the resulting normalized wage measures under double selection (red circles), single selection in levels (black diamonds) and proportional single selection (blue triangles). The first vertical red dashed line indicates the minimum wage increase in California; the second indicates the federal minimum wage increase or the increase of the state level minimum wage (whichever comes first). Hollow markers indicate years in which workers in the control group are subject to the higher minimum wage. Whiskers in the event study plots show 5th and 95th percentiles of 500 bootstraps.