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When Should We Use Unit Fixed Effects Regression Models for Causal Inference with Longitudinal Data?

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Abstract: *Many researchers use unit fixed effects regression models as their default methods for causal inference with longitudinal data. We show that the ability of these models to adjust for unobserved time-invariant confounders comes at the expense of dynamic causal relationships, which are permitted under an alternative selection-on-observables approach. Using the nonparametric directed acyclic graph, we highlight two key causal identification assumptions of unit fixed effects models: Past treatments do not directly influence current outcome, and past outcomes do not affect current treatment. Furthermore, we introduce a new nonparametric matching framework that elucidates how various unit fixed effects models implicitly compare treated and control observations to draw causal inference. By establishing the equivalence between matching and weighted unit fixed effects estimators, this framework enables a diverse set of identification strategies to adjust for unobservables in the absence of dynamic causal relationships between treatment and outcome variables. We illustrate the proposed methodology through its application to the estimation of GATT membership effects on dyadic trade volume.*

Replication Materials: The data, code, and any additional materials required to replicate all analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: https://doi.org/10.7910/DVN/YUM3K8.

Unit fixed effects regression models are widely used for causal inference with longitudinal or panel data in the social sciences (e.g., Angrist and Pischke 2009). Many researchers use these models to adjust for unobserved, unit-specific and time-invariant confounders when estimating causal effects from observational data. In spite of this widespread practice, much methodological discussion of unit fixed effects models in political science has taken place from model-based perspectives (often assuming linearity), with little attention to the causal identification assumptions (e.g., Beck 2001; Bell and Jones 2015; Clark and Linzer 2015; Wilson and Butler 2007). In contrast, our work builds upon a small literature on the use of linear fixed effects models for causal inference with longitudinal data in econometrics and statistics (e.g., Arkhangelsky and Imbens 2018; Sobel 2006; Wooldridge 2005a).

Specifically, we show that the ability of unit fixed effects regression models to adjust for unobserved time-invariant confounders comes at the expense of dynamic causal relationships between treatment and outcome variables, which are allowed to exist under an alternative selection-on-observables approach (e.g., Robins, Hernán, and Brumback 2000). Our analysis highlights two key

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The methods described in this article can be implemented via the open-source statistical software `wfe: Weighted Linear Fixed Effects Estimators for Causal Inference`, available through the Comprehensive R Archive Network (http://cran.r-project.org/package=wfe). The initial draft of this article was entitled "On the Use of Linear Fixed Effects Regression Estimators for Causal Inference" (July 2011). We thank Alberto Abadie, Mike Bailey, Neal Beck, Matias Cattaneo, Naoki Egami, Erin Hartman, Danny Hidalgo, Rocio Titiunik, Yuki Shiraito, and Teppei Yamamoto for helpful comments.

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causal identification assumptions that are required under fixed effects models and yet are often overlooked by applied researchers: (1) past treatments do not directly influence current outcome, and (2) past outcomes do not affect current treatment. Unlike most of the existing discussions of unit fixed effects regression models that assume linearity, we use the directed acyclic graph (DAG) framework (Pearl 2009) that can represent a wide class of nonparametric structural equation models, encompassing linear and other unit fixed effects models as special cases.

In addition, we propose a new analytical framework that directly connects unit fixed effects models to matching methods (e.g., Ho et al. 2007; Rubin 2006; Stuart 2010). The framework makes explicit how counterfactual outcomes are estimated under unit fixed effects regression models. A simple but important insight is that the comparison of treated and control observations must occur within the same unit and across time periods in order to adjust for unobserved, unit-specific and time-invariant confounders. We establish this fact by proving the equivalence between within-unit matching estimators and weighted linear unit fixed effects regression estimators. In particular, the result implies that the counterfactual outcome for a treated observation in a given time period is estimated using the observed outcomes of different time periods of the same unit. Since such a comparison is valid only when no causal dynamics exist, this finding underscores the important limitation of linear regression models with unit fixed effects.

Although linear unit fixed effects models must assume the absence of causal dynamics to adjust for unobserved time-invariant confounders, we further improve these models by relaxing the linearity assumption. Our matching framework incorporates a diverse set of identification strategies to address unobservables under the assumption that the treatment and outcome variables do not influence each other over time. We also derive the weighted linear unit fixed effects regression estimator that is equivalent to a within-unit matching estimator. This equivalence allows us to construct simple model-based standard errors instead of more complex and computationally intensive standard errors proposed in the literature (e.g., Abadie and Imbens 2006, 2012; Otsu and Rai 2017). In addition, we can use the model-based specification test to assess the appropriateness of the linearity assumption in unit fixed effects regression models (White 1980a). Our theoretical results also extend the weighted regression results available in the literature for causal inference with cross-section data to longitudinal studies (e.g., Aronow and Samii 2015; Humphreys 2009; Solon, Haider, and Wooldridge 2015). The proposed methodology is freely available as an R package, `wfe: Weighted Linear Fixed Effects Estimators for Causal Inference`, at the Comprehensive R Archive Network (https://cran.r-project.org/package=wfe).

Finally, we illustrate the proposed methodology by applying it to the controversy regarding the causal effects of General Agreement on Tariffs and Trade (GATT) membership on dyadic trade (Rose 2004; Tomz, Goldstein, and Rivers 2007). Despite the substantive disagreement, there exists a remarkable methodological consensus among researchers in the literature, all of whom endorse the use of linear fixed effects regression models. We critically examine the causal identification assumptions of the models used in previous studies and also consider an alternative identification strategy. We show that the empirical conclusions are highly dependent on the choice of causal identification assumptions.

Due to space constraints, this article does not study linear regression models with unit and time fixed effects (i.e., two-way linear fixed effects models). Imai, Kim, and Wang (2018) further extend our matching framework to these models. The two-way linear fixed effects models are closely related to the difference-in-differences (DiD) identification strategy. The DiD estimator is based on the comparison of treated and control units within the same time period under the assumption of parallel time trend between the treated and control units. Formally, Imai, Kim, and Wang (2018) show that a class of matching estimators, which apply the DiD estimator after adjusting for treatment history and confounders, is equivalent to the weighted two-way linear fixed effects models and derives the exact formula for regression weights. Thus, unlike linear regression models with unit fixed effects, two-way fixed effects models can allow for causal dynamics under the additional assumption of parallel trend.

Causal Identification Assumptions

We study the causal identification assumptions of regression models with unit fixed effects. While we begin our discussion by describing the basic linear regression model with unit fixed effects, our analysis is conducted under a more general, nonparametric setting based on the directed acyclic graphs (DAGs) and potential outcomes frameworks (Imbens and Rubin 2015; Pearl 2009). We show that the ability of unit fixed effects models to adjust for unobserved time-invariant confounders comes

at the expense of dynamic causal relationships between treatment and outcome variables.

The Linear Unit Fixed Effects Regression Model

Throughout this article, for the sake of simplicity, we assume a balanced longitudinal data set of N units and T time periods with no missing data. We also assume a simple random sampling of units from a population with T fixed. For each unit i at time t, we observe the outcome variable Y_{it} and the binary treatment variable $X_{it} \in \{0, 1\}$. The most basic linear regression model with unit fixed effects is based on the following specification:

$$Y_{it} = \alpha_i + \beta X_{it} + \epsilon_{it}, \tag{1}$$

for each $i = 1, 2, \ldots, N$ and $t = 1, 2, \ldots, T$, where α_i is a fixed but unknown intercept for unit i and ϵ_{it} is a disturbance term for unit i at time t with $\mathbb{E}(\epsilon_{it}) = 0$. In this model, the unit fixed effect α_i captures a vector of unobserved time-invariant confounders in a flexible manner. That is, we define each fixed effect as $\alpha_i = h(\mathbf{U}_i)$, where $\mathbf{U}_i$ represents a vector of unobserved time-invariant confounders and $h(\cdot)$ is an arbitrary and unknown function.

Typically, the strict exogeneity of the disturbance term ϵ_{it} is assumed to identify β. Formally, this assumption can be written as

$$\mathbb{E}(\epsilon_{it} \mid \mathbf{X}_i, \alpha_i) = 0, \tag{2}$$

for each $i = 1, 2, \ldots, N$, and $t = 1, 2, \ldots, T$, where $\mathbf{X}_i$ is a $T \times 1$ vector of treatment variables for unit i. Since α_i can be any function of $\mathbf{U}_i$, this assumption is equivalent to $\mathbb{E}(\epsilon_{it} \mid \mathbf{X}_i, \mathbf{U}_i) = 0$.

We refer to this model based on Equations 1 and 2 as $\mathsf{LIN-FE}$. The least squares estimate of β is obtained by regressing the deviation of the outcome variable from its mean on the deviation of the treatment variable from its mean:

$$\hat{\beta}_{\mathsf{LIN-FE}} = \operatorname*{argmin}_{\beta} \sum_{i=1}^{N} \sum_{t=1}^{T} \left\{ (Y_{it} - \overline{Y}_i) - \beta (X_{it} - \overline{X}_i) \right\}^2, \tag{3}$$

where $\overline{X}_i = \sum_{t=1}^{T} X_{it}/T$ and $\overline{Y}_i = \sum_{t=1}^{T} Y_{it}/T$. If the data are generated according to $\mathsf{LIN-FE}$, then $\hat{\beta}_{\mathsf{LIN-FE}}$ is unbiased for β.

The parameter β is interpreted as the average contemporaneous effect of X_{it} on Y_{it}. Formally, let $Y_{it}(x)$ represent the potential outcome for unit i at time t under the treatment status $X_{it} = x$ for $x = 0, 1$, where the observed outcome equals $Y_{it} = Y_{it}(X_{it})$. Equation 3 shows that units with no variation in the treatment variable do not contribute to the estimation of β. Thus, under $\mathsf{LIN-FE}$, the causal estimand is the following average treatment effect among the units with some variation in the treatment status:

$$\tau = \mathbb{E}(Y_{it}(1) - Y_{it}(0) \mid C_i = 1), \tag{4}$$

where $C_i = \mathbf{1}\{0 < \sum_{t=1}^{T} X_{it} < T\}$. Under $\mathsf{LIN-FE}$, this quantity is represented by β (i.e., $\beta = \tau$), because of the assumed linearity for potential outcomes, that is, $Y_{it}(x) = \alpha_i + \beta x + \epsilon_{it}$.

Nonparametric Causal Identification Analysis

To understand the fundamental causal assumptions of unit fixed effects models, we conduct a nonparametric identification analysis that avoids any parametric restriction. Specifically, we relax the linearity assumption of $\mathsf{LIN-FE}$ (i.e., Equation 1). We also generalize mean independence (i.e., Equation 2) to statistical independence. The resulting model is the following nonparametric fixed effects model ($\mathsf{NP-FE}$).

Assumption 1 (Nonparametric Fixed Effects Model). *For each $i = 1, 2, \ldots, N$ and $t = 1, 2, \ldots, T$,*

$$Y_{it} = g(X_{it}, \mathbf{U}_i, \epsilon_{it}) \text{ and} \tag{5}$$

$$\epsilon_{it} \perp\!\!\!\perp \{\mathbf{X}_i, \mathbf{U}_i\}, \tag{6}$$

where $g(\cdot)$ can be any function.

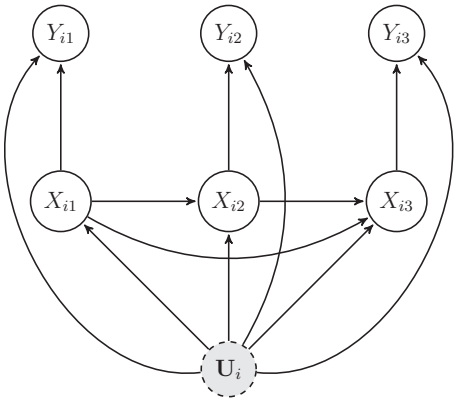
Note that $\mathsf{NP-FE}$ includes $\mathsf{LIN-FE}$ as a special case.[1] Unlike $\mathsf{LIN-FE}$, $\mathsf{NP-FE}$ does not assume a functional form and enables all effects to vary across observations.

We examine causal assumptions of $\mathsf{NP-FE}$ using the directed acyclic graphs (DAGs). Pearl (2009) shows that a DAG can formally represent a nonparametric structural equation model (NPSEM), avoiding functional-form and distributional assumptions while enabling general forms of effect heterogeneity.[2] The DAG in Figure 1 graphically represents the NPSEM that corresponds to $\mathsf{NP-FE}$. For simplicity, the DAG only describes the causal relationships for three time periods, but we assume that the same relationships apply to all time

[1] Although mean independence does not necessarily imply statistical independence, in most cases researchers have no substantive justification to assume the former rather than the latter. Under the assumption of statistical independence, $\mathsf{LIN-FE}$ is a special case of $\mathsf{NP-FE}$.

[2] More precisely, $\mathsf{NP-FE}$ implies $X_{it} = f(X_{i1}, \ldots, X_{i,t-1}, \mathbf{U}_i, \eta_{it})$, where η_{it} is an exogenous disturbance term and $f(\cdot)$ is any function.

FIGURE 1 Directed Acyclic Graph for Regression Models with Unit Fixed Effects Based on Three Time Periods



Note: Solid circles represent observed outcome Y_{it} and treatment X_{it} variables, whereas a gray dashed circle represents a vector of unobserved, unit-specific and time-invariant confounders $\mathbf{U}_i$. The solid arrows indicate the possible existence of causal relationships, whereas the absence of such arrows represents the lack of causal relationships. DAGs are also assumed to contain all relevant, observed and unobserved, variables.

periods even when there are more than three time periods (i.e., $T > 3$). In this DAG, the observed variables, X_{it} and Y_{it}, are represented by solid circles, whereas a dashed gray circle represents the unobserved time-invariant confounders, $\mathbf{U}_i$. The solid black arrows indicate the possible existence of direct causal effects, whereas the absence of such arrows represents the assumption of no direct causal effect. In addition, DAGs are assumed to contain all relevant, observed and unobserved, variables. Therefore, this DAG assumes the absence of unobserved time-varying confounders.

The DAG in Figure 1 shows that Assumption 1 of NP − FE can be understood as the following set of statements, each of which is represented by the absence of corresponding nodes or arrows:

Assumption (a) No unobserved time-varying confounder exists.
Assumption (b) Past outcomes do not directly affect current outcome.
Assumption (c) Past outcomes do not directly affect current treatment.
Assumption (d) Past treatments do not directly affect current outcome.

No additional arrows can be added to the DAG without making it inconsistent with NP − FE. In particular, no additional arrows that point to X_{it} can be included in the DAG without violating the strict exogeneity assumption of ϵ_{it} under NP − FE. The existence of any such arrow, which must originate from past outcomes $Y_{it'}$ where $t' < t$, would imply a possible correlation between $\epsilon_{it'}$ and X_{it}.[3]

Next, we adopt the potential outcomes framework. While DAGs illuminate the entire causal structure, the potential outcomes framework clarifies the assumptions about treatment assignment mechanisms. First, the right-hand sides of Equations 1 and 5 include the contemporaneous value of the treatment but not its past values, implying that past treatments do not directly affect current outcome. We call this restriction the assumption of no carryover effect,[4] corresponding to Assumption (d) described above.

Assumption 2 (No carryover effect). *For each* $i = 1, 2, \ldots, N$ *and* $t = 1, 2, \ldots, T$, *the potential outcome is given by*

$$Y_{it}(X_{i1}, X_{i2}, \ldots, X_{i,t-1}, X_{it}) = Y_{it}(X_{it}).$$

To better understand the assumed treatment assignment mechanism, we consider a randomized experiment to which NP − FE is applicable. This experiment can be described as follows: For any given unit i, we randomize the treatment X_{i1} at time 1, and for the next time period 2, we randomize the treatment X_{i2} conditional on the realized treatment at time 1—that is, X_{i1}, but without conditioning on the previous outcome Y_{i1}. More generally, at time t, we randomize the current treatment X_{it} conditional on the past treatments $X_{i1}, X_{i2}, \ldots, X_{i,t-1}$. The critical assumptions are that there exists no unobserved time-varying confounder (Assumption a) and that the treatment assignment probability at time t cannot depend on its past realized outcomes $Y_{it'}$ where $t' < t$ (Assumption c). However, the treatment assignment probability may vary across units as a function of unobserved time-invariant characteristics $\mathbf{U}_i$. We can formalize this treatment assignment mechanism as follows.

[3]This is because Y_{it} acts as a collider on any path between ϵ_{it} and $\{\mathbf{X}_i, \mathbf{U}_i\}$.

[4]These models are based on the usual assumption of no spillover effect that the outcome of a unit is not affected by the treatments of other units (Rubin 1990). The assumption of no spillover effect is made throughout this article.

Assumption 3 (Sequential Ignorability with Unobserved Time-Invariant Confounders.). *For each* $i = 1, 2, \ldots, N$,

$$\{Y_{it}(1), Y_{it}(0)\}_{t=1}^{T} \perp\!\!\!\perp X_{i1} \mid \mathbf{U}_i,$$

$$\vdots$$

$$\{Y_{it}(1), Y_{it}(0)\}_{t=1}^{T} \perp\!\!\!\perp X_{it'} \mid X_{i1}, \ldots, X_{i,t'-1}, \mathbf{U}_i,$$

$$\vdots$$

$$\{Y_{it}(1), Y_{it}(0)\}_{t=1}^{T} \perp\!\!\!\perp X_{it} \mid X_{i1}, \ldots, X_{i,T-1}, \mathbf{U}_i.$$

Thus, Assumption 2 corresponds to Assumption d of NP − FE and is implied by equation 1 of LIN − FE. In addition, Assumption 3 corresponds to Assumptions (a) and (c) of NP − FE and the strict exogeneity assumption of LIN − FE given in Equation 2 (see Appendix A.1 for a proof).

Which Causal Identification Assumptions Can Be Relaxed?

It is well known that the assumption of no unobserved time-varying confounder (Assumption a) is difficult to relax under the fixed effects models. Therefore, we consider the other three identification assumptions shared by LIN − FE and NP − FE (Assumptions b, c, and d) in turn.

First, note that we did not mention Assumption (b) under the potential outcomes framework. Indeed, this assumption—past outcomes do not directly affect current outcome—can be relaxed without compromising causal identification. To see this, suppose that past outcomes directly affect current outcome as in Figure 2(a). Even under this scenario, past outcomes do not confound the causal relationship between current treatment and current outcome so long as we condition on past treatments and unobserved time-invariant confounders. The reason is that past outcomes do not directly affect current treatment. Thus, there is no need to adjust for past outcomes even when they directly affect current outcome.[5] The existence of such a relationship, however, may necessitate the adjustment of standard errors, for example, via-cluster robust standard errors.

Next, we entertain the scenario in which past treatments directly affect current outcome (i.e., relaxing Assumption d). Typically, applied researchers address this possibility by including lagged treatment variables in LIN − FE. Here, we consider the following model with one time period lag:

$$Y_{it} = \alpha_i + \beta_1 X_{it} + \beta_2 X_{i,t-1} + \epsilon_{it}. \tag{7}$$

The model implies that the potential outcome can be written as a function of the contemporaneous and previous treatments, that is, $Y_{it}(X_{i,t-1}, X_{it})$, rather than the contemporaneous treatment alone, partially relaxing Assumption 2.

The DAG in Figure 2(b) generalizes the above model and depicts an NPSEM in which a treatment possibly affects all future outcomes as well as current outcome. This NPSEM is a modification of NP − FE, replacing Equation 5 with the following alternative model for the outcome:

$$Y_{it} = g(X_{i1}, \ldots, X_{it}, \mathbf{U}_i, \epsilon_{it}). \tag{8}$$

It can be shown that under this NPSEM, Assumption 3 still holds.[6] The only difference between the DAGs in Figures 1 and 2(b) is that in the latter, we must adjust for the past treatments because they confound the causal relationship between the current treatment and outcome.

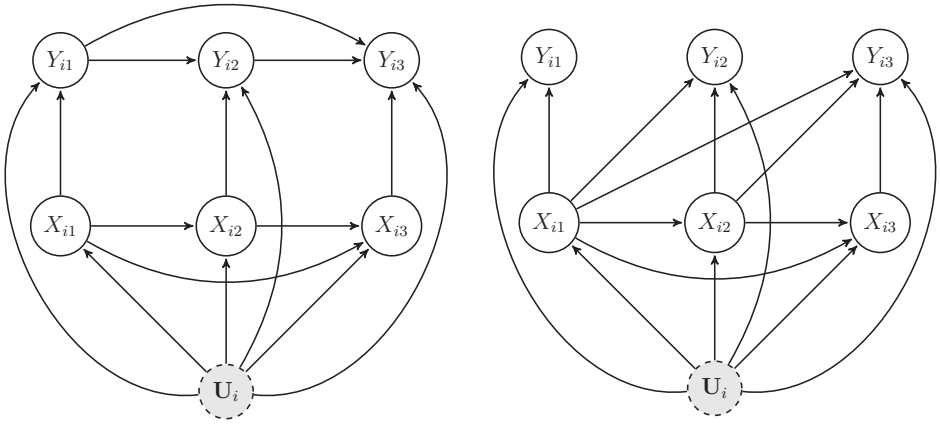
In general, however, we cannot nonparametrically adjust for all past treatments and unobserved time-invariant confounders $\mathbf{U}_i$ at the same time. By nonparametric adjustment, we mean that researchers match exactly on confounders. To nonparametrically adjust for $\mathbf{U}_i$, the comparison of treated and control observations must be done across different time periods within the same unit. The problem is that no two observations within a unit, measured at different time periods, share the same treatment history. Such adjustment must be done by comparing observations across units within the same time period, and yet doing so makes it impossible to adjust for unobserved time-invariant variables.

Therefore, in practice, researchers assume that only a small number of past treatments matter. Indeed, a frequent practice is to adjust for one time period lag. Under this assumption, multiple observations within the same unit may share the identical but partial treatment history even though they are measured at different points in time. Under the linear regression framework, researchers conduct a parametric adjustment by simply including a small number of past treatments, as done in Equation 7. However, typically the number of lagged treatments to

[5] The application of the adjustment criteria (Shpitser, VanderWeele, and Robins 2010) implies that these additional causal relationships do not violate Assumption 3 since every noncausal path between the treatment X_{it} and any outcome $Y_{it'}$ is blocked where $t \neq t'$.

[6] The result follows from the application of the adjustment criteria (Shpitser, VanderWeele, and Robins 2010), in which any noncausal path between ϵ_{it} and $\{\mathbf{X}_i, \mathbf{U}_i\}$ contains a collider Y_{it}. This result also holds even if past outcomes affect current outcomes (i.e., without Assumption b).

FIGURE 2 Identification Assumptions of Regression Models with Unit Fixed Effects



(a) past outcome affects current outcome

(b) past treatments affect current outcome

(c) past outcomes affect current treatment

(d) past outcomes affect both current outcome and treatment

Note: Identification is not compromised when past outcomes affect current outcome (panel a). However, the other two scenarios (panels b and c) violate the strict exogeneity assumption. To address the possible violation of strict exogeneity shown in panel (c), researchers often use an instrumental variable approach, shown in panel (d), in which past outcomes affect both current outcome and treatment but Y_{i1} is assumed not to directly affect Y_{i3}.

be included is arbitrarily chosen and is rarely justified on substantive grounds.[7]

Finally, we consider relaxing the assumption that past outcomes do not directly affect current treatment (Assumption c). This scenario is depicted as Figure 2(c). It is immediate that Assumption 3 is violated because the existence of causal relationships between past outcomes and current treatment implies a correlation between past disturbance terms and current treatment.[8] This lack of feedback effects over time represents another key causal assumption required for the unit fixed effects models.

To address this issue, the model that has attracted much attention is the following linear unit fixed effects model with a lagged outcome variable:

$$Y_{it} = \alpha_i + \beta X_{it} + \rho Y_{i,t-1} + \epsilon_{it}. \tag{9}$$

[7]One exception is the setting where the treatment status changes only once in the same direction (e.g., from the control to treatment condition). While adjusting for the previous treatment is sufficient in this case, there may exist a time trend in outcome, which confounds the causal relationship between treatment and outcome.

[8]For example, there is an unblocked path from X_{i2} to ϵ_{i1} through Y_{i1}.

TABLE 1 Identification Assumptions of Various Estimators

	Linearity	Time-Invariant Unobservables	Past Outcomes Affect Current Treatment	Past Treatments Affect Current Outcome
$Y_{it} = \alpha_i + \beta X_{it} + \epsilon_{it}$	Yes	Allowed	Not allowed	Not allowed
$Y_{it} = \alpha_i + \beta X_{it} + \rho Y_{i,t-1} + \epsilon_{it}$	Yes	Allowed	Allowed	Not allowed
$Y_{it} = \alpha_i + \beta_1 X_{it} + \beta_2 X_{i,t-1} + \epsilon_{it}$	Yes	Allowed	Not allowed	Allowed
$Y_{it} = \alpha_i + \beta_1 X_{it} + \beta_2 X_{i,t-1} + \rho Y_{i,t-1} + \epsilon_{it}$	Yes	Allowed	Partially allowed	Partially allowed
Marginal structural models	No	Not allowed	Allowed	Allowed

Figure 2(d) presents a DAG that corresponds to this model. The standard identification strategy commonly employed for this model is based on instrumental variables (e.g., Arellano and Bond 1991). The results of Brito and Pearl (2002) imply that we can identify the average causal effect of X_{i3} on Y_{i3} by using X_{i1}, X_{i2}, and Y_{i1} as instrumental variables while conditioning on $\mathbf{U}_i$ and Y_{i2}.[9] However, the validity of each instrument depends on the assumed absence of its direct causal effect on the outcome variable (i.e., direct effects of Y_{i1}, X_{i1}, and X_{i2} on Y_{i3}). Unfortunately, in practice, these assumptions are often made without a substantive justification.

In sum, three key causal identification assumptions are required for $\mathsf{LIN}-\mathsf{FE}$ and its nonparametric generalization $\mathsf{NP}-\mathsf{FE}$. The assumption of no unobserved time-varying confounder is well appreciated by applied researchers. However, many fail to recognize two additional assumptions required for the unit fixed effects regression models: Past treatments do not affect current outcome and past outcomes do not affect current treatment. The former can be partially relaxed by assuming that only a small number of lagged treatment variables affect the outcome. The use of instrumental variables is a popular approach to relax the latter assumption, but under this approach we must instead assume that some lagged outcome variables do not directly affect current outcome. Unfortunately, researchers rarely justify these alternative identification strategies on substantive grounds.

Finally, although this article focuses upon models with time-invariant unobservables, we emphasize a key trade-off between causal dynamics and time-invariant unobservables. An alternative selection-on-observable approach allows for the existence of such dynamic causal relationships even though it assumes the absence of time-invariant unobservables (see Robins, Hernán, and Brumback 2000).[10] The key decision for applied researchers is then whether, in a given substantive problem, they believe causal dynamics is more important than time-invariant unobserved confounders.

Table 1 summarizes the identification assumptions of various estimators considered in this section. The standard linear regression model with unit fixed effects allows for the existence of time-invariant unobservables but does not allow causal dynamics. By including lagged outcome and treatment variables, one can allow either past outcomes to affect current treatment or past treatments to affect current outcome. However, allowing both types of causal dynamics requires an instrumental variable approach, in which past outcomes, after a certain number of lags, are assumed not to directly affect current outcome. Finally, the selection-on-observable approach based on marginal structural models can nonparametrically identify causal effects in the presence of causal dynamics, but this is done under the assumption of no time-invariant unobservables.

Adjusting for Observed Time-Varying Confounders

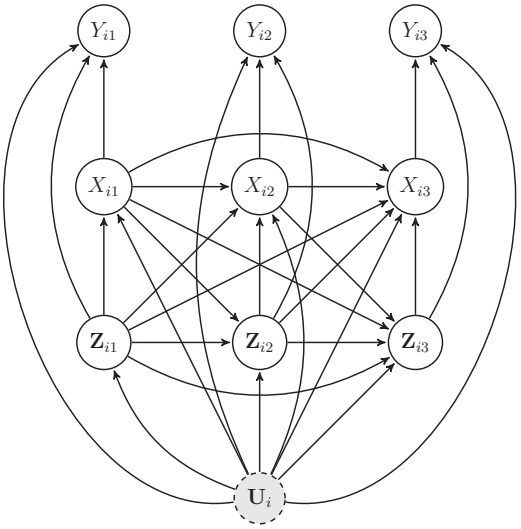
Finally, we consider the adjustment of observed time-varying confounders under fixed effects regression models. Since fixed effects models can only adjust for unobserved confounders that are time-invariant, applied researchers often measure a vector of observed time-varying confounders $\mathbf{Z}_{it}$ to improve the credibility of assumptions.[11] We show here that the main conclusion of the above identification analysis remains unchanged even if we include these additional observed time-varying confounders as covariates of the fixed effects regression

[9] If X_{i1} and X_{i2} directly affect Y_{i3}, then only Y_{i1} can serve as a valid instrument. If past outcomes do not affect current outcome, then we can use both Y_{i1} and Y_{i2} as instruments.

[10] See Blackwell (2013) and Blackwell and Glynn (2018) for recent articles that introduce relevant modeling techniques under the assumption of selection-on-observables to political science.

[11] Note that $\mathbf{Z}_{it}$ is assumed to be causally prior to the current treatment X_{it}.

FIGURE 3 Directed Acyclic Graph for Regression Models with Unit Fixed Effects and Observed Time-Varying Confounders Based on Three Time Periods



models. In fact, in this case, we must make an additional assumption that there exists no dynamic causal relationship between outcome and these time-varying confounders, leading to potentially even less credible causal identification in many applications.

Consider the following NP − FE, which now includes observed time-varying confounders $\mathbf{Z}_{it}$.

Assumption 4. (NONPARAMETRIC FIXED EFFECTS MODEL WITH OBSERVED TIME-VARYING CONFOUNDERS). *For each* $i = 1, 2, \ldots, N$ *and* $t = 1, 2, \ldots, T$,

$$Y_{it} = g(X_{i1}, \ldots, X_{it}, \mathbf{U}_i, Z_{i1}, \ldots, Z_{it}, \boldsymbol{\epsilon}_{it}) \text{ and } \quad (10)$$

$$\boldsymbol{\epsilon}_{it} \perp\!\!\!\perp \{\mathbf{X}_i, \mathbf{Z}_i, \mathbf{U}_i\}, \quad (11)$$

where $\mathbf{Z}_i = (\mathbf{Z}_{i1}\ \mathbf{Z}_{i2}\ \ldots\ \mathbf{Z}_{it})$.

Figure 3 presents a DAG that corresponds to a nonparametric structural equation model consistent with this NP − FE. The difference between the DAGs shown in Figures 1 and 3 is the addition of $\mathbf{Z}_{it}$, which directly affects the contemporaneous outcome Y_{it}, the current and future treatments $\{X_{it}, X_{i,t+1}, \ldots, X_{it}\}$, and their own future values $\{\mathbf{Z}_{i,t+1}, \ldots, \mathbf{Z}_{it}\}$. Moreover, the unobserved time-invariant confounders $\mathbf{U}_i$ can directly affect these observed time-varying confounders. Under this model, only the contemporaneous time-varying confounders $\mathbf{Z}_{it}$ and the unobserved time-invariant confounders $\mathbf{U}_i$ confound the contemporaneous causal relationship between X_{it} and Y_{it}. Neither past treatments nor past time-varying confounders need to be adjusted because they do not directly affect current outcome Y_{it}.

Now, suppose that the observed time-varying confounders $\mathbf{Z}_{it}$ directly affect future and current outcomes $Y_{it'}$ where $t' \geq t$. In this case, we need to adjust for the past values of the observed time-varying confounders as well as their contemporaneous values. This can be done by including the relevant lagged confounding variables, (that is, $\mathbf{Z}_{it'}$ with $t' < t$, in fixed effects regression models. However, for the same reason as the one explained in the last subsection, it is impossible to nonparametrically adjust for the entire sequence of past time-varying confounders and unobserved time-invariant confounders $\mathbf{U}_i$ at the same time. While the nonparametric adjustment of $\mathbf{U}_i$ requires the comparison of observations across different time periods within each unit, no two observations measured at different points in time share an identical history of time-varying confounders.

Furthermore, similar to the case of NP − FE without time-varying confounders, the average contemporaneous treatment effect of X_{it} on Y_{it} becomes unidentifiable if the outcome Y_{it} affects future treatments $X_{it'}$ either directly or indirectly through $\mathbf{Z}_{it'}$ where $t' > t$. This is because the existence of a causal relationship between Y_{it} and $\mathbf{Z}_{it'}$ implies a correlation between $\boldsymbol{\epsilon}_{it}$ and $\mathbf{Z}_{it'}$, thereby violating Assumption 4. In the previous subsection, we pointed out the difficulty of assuming the lack of causal relationships between past outcomes and current treatment. In many applications, we expect feedback effects to occur over time between outcome and treatment variables. For the same reason, assuming the absence of causal effects of past outcomes on current time-varying confounders may not be realistic.

The above discussion implies that researchers face the same key trade-off regardless of whether time-varying confounders are present. To adjust for unobserved time-invariant confounders, researchers must assume the absence of dynamic causal relationships among the outcome, treatment, and observed time-varying confounders. In contrast, the selection-on-observables approach can relax these assumptions so long as there exists no unobserved time-invariant confounder. Under this alternative approach, past treatments can directly affect current outcome, and past outcomes can either directly or indirectly affect current treatment (through time-varying confounders).

In the next section, we propose a within-unit matching estimator, which relaxes the common assumption of linearity among unit fixed effects estimators discussed in this section. Although the fundamental trade-off between causal dynamics and time-invariant unobservables is unavoidable, relaxing the functional form assumption

of linear regression models with unit fixed effects enables more robust inference when the identification assumptions are met.

A New Matching Framework

Causal inference is all about the question of how to credibly estimate the counterfactual outcomes through the comparison of treated and control observations. For a treated observation, we observe the outcome under the treatment condition but must infer its counterfactual outcome under the control condition using the observed outcomes of control observations. Matching is a class of nonparametric methods in which we solve this fundamental problem of causal inference by finding a set of control observations similar to each treated observation (e.g., Ho et al. 2007; Rubin 2006; Stuart 2010).

In this section, we propose a new matching framework to shed new light on the causal identification assumptions of unit fixed effects regression models. Like the DAGs introduced above, this new matching framework is completely nonparametric. In the following subsection, we show that relaxing the linearity assumption is key to consistently estimating the causal quantity of interest defined in Equation 4. Moreover, the proposed framework elucidates how various unit fixed effects models adjust for time-invariant unobservables. Specifically, the subsequent section shows that causal inference based on unit fixed effects regression models relies upon within-unit comparison where a treated observation is matched with the control observations of the same unit at different time periods. We then establish this fact by proving that a within-unit matching estimator is equivalent to a weighted linear unit fixed effects regression model.

Consistent with the earlier analysis, such over-time comparison is valid only in the absence of causal dynamics. However, the proposed nonparametric matching framework can serve as an important foundation for simultaneously relaxing the linearity assumption and enabling a variety of identification strategies based on within-unit comparison in the presence of unobservable unit-specific heterogeneity.

The Within-Unit Matching Estimator

Despite its popularity, $\mathsf{LIN}-\mathsf{FE}$ does not consistently estimate the average treatment effect (ATE) defined in Equation 4 even when Assumptions 2 and 3 are satisfied. This is because $\mathsf{LIN}-\mathsf{FE}$ additionally requires the linearity assumption. Indeed, the linear unit fixed effects regression estimator given in Equation 3 converges to a weighted average of unit-specific ATEs in which the weights are proportional to the within-unit variance of the treatment assignment variable. This result is restated here as a proposition.

Proposition 1 (Inconsistency of the Linear Fixed Effects Regression Estimator [Chernozhukov et al. 2013, Theorem 1]). *Suppose* $\mathbb{E}(Y_{it}^2) < \infty$ *and* $\mathbb{E}(C_i S_i^2) > 0$ *where* $S_i^2 = \sum_{t=1}^T (X_{it} - \overline{X}_i)^2/(T-1)$. *Under Assumptions 2 and 3 as well as simple random sampling of units with* T *fixed, the linear fixed effects regression estimator given in Equation* 3 *is inconsistent for the average treatment effect* τ *defined in Equation* 4*:*

$$\hat{\beta}_{\mathsf{LIN-FE}} \xrightarrow{p} \frac{\mathbb{E}\left\{ C_i \left(\frac{\sum_{t=1}^T X_{it} Y_{it}}{\sum_{t=1}^T X_{it}} - \frac{\sum_{t=1}^T (1-X_{it}) Y_{it}}{\sum_{t=1}^T 1 - X_{it}} \right) S_i^2 \right\}}{\mathbb{E}(C_i S_i^2)} \neq \tau.$$

Thus, in general, under Assumptions 2 and 3, the linear unit fixed effects estimator fails to consistently estimate the ATE unless either the within-unit ATE or the within-unit proportion of treated observations is constant across units. This result also applies to the use of $\mathsf{LIN}-\mathsf{FE}$ in a cross-sectional context. For example, $\mathsf{LIN}-\mathsf{FE}$ is often used to analyze stratified randomized experiments (Duflo, Glennerster, and Kremer 2007). Even in this case, if the proportion of treated observations and the ATE vary across strata, then the resulting least squares estimator will be inconsistent.

We consider a nonparametric matching estimator that eliminates this bias. The key insight from an earlier discussion is that under Assumptions 2 and 3, even though a set of time-invariant confounders $\mathbf{U}_i$ is not observed, we can nonparametrically adjust for them by comparing the treated and control observations measured at different time periods within the same unit. This within-unit comparison motivates the following matching estimator, which computes the difference of means between the treated and control observations within each unit and then averages it across units:

$$\hat{\tau} = \frac{1}{\sum_{i=1}^N C_i} \sum_{i=1}^N C_i \left(\frac{\sum_{t=1}^T X_{it} Y_{it}}{\sum_{t=1}^T X_{it}} - \frac{\sum_{t=1}^T (1-X_{it}) Y_{it}}{\sum_{t=1}^T (1-X_{it})} \right). \quad (12)$$

This matching estimator is attractive because unlike the estimator in Equation 3, it does not require the linearity assumption. Under Assumptions 2 and 3, this within-unit

matching estimator is consistent for the ATE defined in Equation 4.

Proposition 2 (Consistency of the Within-Unit Matching Estimator). *Under the same set of assumptions as in Proposition 1, the within-unit matching estimator defined in Equation* 12 *is consistent for the average treatment effect defined in Equation* 4.

Proof is in Appendix A.2.

To further generalize this idea, we make the connection to matching methods more explicit by defining a *matched set* $\mathcal{M}_{it}$ for each observation (i, t) as a group of other observations that are matched with it. For example, under the estimator proposed above, a treated (control) observation is matched with all control (treated) observations within the same unit, and hence the matched set is given by

$$\mathcal{M}_{it} = \{(i', t') : i' = i, X_{i't'} = 1 - X_{it}\}. \quad (13)$$

Thus far, we have focused on the average treatment effect as a parameter of interest given that researchers often interpret the parameter β of $\mathsf{LIN-FE}$ as the average contemporaneous treatment effect of X_{it} on Y_{it}. However, our matching framework can accomodate various identification strategies for different causal quantities of interest using different matched sets. That is, given any matched set $\mathcal{M}_{it}$, we can define the corresponding within-unit matching estimator $\hat{\tau}$ as

$$\hat{\tau} = \frac{1}{\sum_{i=1}^{N}\sum_{t=1}^{T} D_{it}} \sum_{i=1}^{N}\sum_{t=1}^{T} D_{it}(\widehat{Y_{it}(1)} - \widehat{Y_{it}(0)}), \quad (14)$$

where $Y_{it}(x)$ is observed when $X_{it} = x$ and is estimated using the average of outcomes among the units of its matched set when $X_{it} = 1 - x$:

$$\widehat{Y_{it}(x)} = \begin{cases} Y_{it} & \text{if } X_{it} = x \\ \frac{1}{|\mathcal{M}_{it}|}\sum_{(i',t')\in\mathcal{M}_{it}} Y_{i't'} & \text{if } X_{it} = 1 - x. \end{cases} \quad (15)$$

Note that $|\mathcal{M}_{it}|$ represents the number of observations in the matched set and that D_{it} indicates whether the matched set $\mathcal{M}_{it}$ contains at least one observation, that is, $D_{it} = \mathbf{1}\{|\mathcal{M}_{it}| > 0\}$. In the case of the matched set defined in Equation 13, we have $D_{it} = C_i$ for any t.

Identification Strategies Based on Within-Unit Comparison

The framework described above can accommodate diverse matching estimators through their corresponding matched sets $\mathcal{M}_{it}$. Here, we illustrate the generality of the proposed framework. First, we show how to incorporate time-varying confounders $\mathbf{Z}_{it}$ by matching observations within each unit based on the values of $\mathbf{Z}_{it}$. For example, the within-unit nearest neighbor matching leads to the following matched set:

$$\mathcal{M}_{it} = \{(i', t') : i' = i, X_{i't'} = 1 - X_{it}, \mathcal{D}(\mathbf{Z}_{it}, \mathbf{Z}_{i't'}) = J_{it}\}, \quad (16)$$

where $\mathcal{D}(\cdot, \cdot)$ is a distance measure (e.g., Mahalanobis distance), and

$$J_{it} = \min_{(i,t')\in\mathcal{M}_{it}} \mathcal{D}(\mathbf{Z}_{it}, \mathbf{Z}_{it'}) \quad (17)$$

represents the minimum distance between the time-varying confounders of this observation and another observation from the same unit whose treatment status is opposite. With this definition of matched set, we can construct the within-unit nearest neighbor matching estimator using Equation 14. The argument of Proposition 2 suggests that this within-unit nearest neighbor matching estimator is consistent for the ATE so long as matching on $\mathbf{Z}_{it}$ eliminates the confounding bias.

Second, we consider the before-and-after (BA) design in which each average potential outcome is assumed to have no time trend over a short time period. Since the BA design also requires the assumption of no carryover effect, the BA design may be most useful when for a given unit the change in the treatment status happens only once. Under the BA design, we simply compare the outcome right before and immediately after a change in the treatment status. Formally, the assumption of no time trend can be written as the following:

Assumption 5 (Before-and-After Design). *For* $i = 1, 2, \ldots, N$ *and* $t = 2, \ldots, T$,

$$\mathbb{E}(Y_{it}(x) - Y_{i,t-1}(x) \mid X_{it} \neq X_{i,t-1}) = 0,$$

where $x \in \{0, 1\}$.

Under Assumptions 2 and 5, the average difference in outcome between before and after a change in the treatment status is a valid estimate of the local ATE, that is, $\mathbb{E}(Y_{it}(1) - Y_{it}(0) \mid X_{it} \neq X_{i,t-1})$.

To implement the BA design within our framework, we restrict the matched set and compare the observations within two subsequent time periods that have opposite treatment status. Formally, the resulting matched set can

be written as

$$\mathcal{M}_{it} = \{(i', t') : i' = i, t' = t - 1, X_{i't'} = 1 - X_{it}\}. \quad (18)$$

It is straightforward to show that this matching estimator is equivalent to the following first-difference (FD) estimator:

$$\hat{\beta}_{\mathsf{FD}} = \underset{\beta}{\operatorname{argmin}} \sum_{i=1}^{N} \sum_{t=2}^{T} \left\{ (Y_{it} - Y_{i,t-1}) - \beta(X_{it} - X_{i,t-1}) \right\}^2. \quad (19)$$

In standard econometrics textbooks, the FD estimator defined in Equation 19 is presented as an alternative estimation method for the $\mathsf{LIN} - \mathsf{FE}$ estimator. For example, Wooldridge (2010) writes, "We emphasize that the model and the interpretation of β are *exactly* as in [the linear fixed effects model]. What differs is our method for estimating β" (279; italics original). However, as can be seen from the above discussion, the difference lies in the identification assumption and the population for which the ATE is identified. Both the $\mathsf{LIN} - \mathsf{FE}$ and FD estimators match observations within the same unit. However, the FD estimator matches observations only from subsequent time periods, whereas the $\mathsf{LIN} - \mathsf{FE}$ estimator matches observations from all time periods regardless of the treatment status.

There exists an important limitation of the BA design. Although Assumption 5 is written in terms of time trend of potential outcomes, the assumption is violated if past outcomes affect current treatment. Since the first-difference estimator can be thought of as a special case of linear unit fixed effects regression estimators, the critical assumption of no dynamic causal relationships between the outcome and treatment variables remains relevant under the BA design. Consider a scenario where the treatment variable X_{it} is set to 1 when the lagged outcome $Y_{i,t-1}$ takes a value greater than its mean. In this case, even if the treatment effect is 0, the outcome difference between the two periods, $Y_{it} - Y_{i,t-1}$, is likely to be negative because of the so-called "regression toward the mean" phenomenon. James (1973) derives an expression for this bias under the normality assumption.

Finally, we can generalize the BA design so that we use a larger number of lags to estimate the counterfactual outcome. If we let L represent the number of lags, then the matched set becomes

$$\mathcal{M}_{it} = \{(i', t') : i' = i, t' \in \{t - 1, \ldots, t - L\}, X_{i't'} = 1 - X_{it}\}. \quad (20)$$

Furthermore, the causal quantity of interest can also be generalized to a longer-term average treatment effect, that is,

$$\mathbb{E}\{Y_{i,t+F}(1) - Y_{i,t+F}(0) \mid X_{it} \neq X_{i,t-1}\}, \quad (21)$$

where F is a non-negative integer representing the number of leads. Under this setting, we estimate the potential outcome under $X_{it} = x$ at time $t + F$ using the following matching estimator:

$$\hat{\tau} = \frac{1}{\sum_{i=1}^{N} \sum_{t=L+1}^{T-F} D_{it}} \sum_{i=1}^{N} \sum_{t=L+1}^{T-F} D_{it}(\widehat{Y_{i,t+F}(1)} - \widehat{Y_{i,t+F}(0)}), \quad (22)$$

where

$$\widehat{Y_{i,t+F}(x)} = \begin{cases} Y_{i,t+F} & \text{if } X_{it} = 1 - X_{i,t-1} = x \\ \frac{1}{|\mathcal{M}_{it}|} \sum_{(i',t') \in \mathcal{M}_{it}} Y_{i't'} & \text{if } X_{it} = 1 - x \end{cases},$$

$$D_{it} = \begin{cases} 1 & \text{if } X_{it} = 1 - X_{i,t-1} \\ 0 & \text{otherwise.} \end{cases} \quad (23)$$

Estimation, Inference, and Specification Test

As the main analytical result of this article, we show that *any* within-unit matching estimator can be written as a weighted linear regression estimator with unit fixed effects. The following theorem establishes this result and shows how to compute regression weights for a given matched set (see Gibbons, Suárez Serrato, and Urbancic 2017; Solon, Haider, and Wooldridge 2015 for related results).

Theorem 1 (Within-Unit Matching Estimator as a Weighted Unit Fixed Effects Estimator). *Any within-unit matching estimator $\hat{\tau}$ defined by a matched set $\mathcal{M}_{it}$ equals the weighted linear fixed effects estimator, which can be computed as*

$$\hat{\beta}_{\text{WFE}} = \underset{\beta}{\operatorname{argmin}} \sum_{i=1}^{N} \sum_{t=1}^{T} W_{it} \left\{ (Y_{it} - \overline{Y}_i^*) - \beta(X_{it} - \overline{X}_i^*) \right\}^2, \quad (24)$$

where $\overline{X}_i^* = \sum_{t=1}^{T} W_{it} X_{it} / \sum_{t=1}^{T} W_{it}$, $\overline{Y}_i^* = \sum_{t=1}^{T} W_{it} Y_{it} / \sum_{t=1}^{T} W_{it}$, *and the weights are given by*

$$W_{it} = D_{it} \sum_{i'=1}^{N} \sum_{t'=1}^{T} w_{it}^{i't'}, \quad \text{where}$$

$$w_{it}^{i't'} = \begin{cases} 1 & \text{if } (i,t) = (i',t') \\ 1/|\mathcal{M}_{i't'}| & \text{if } (i,t) \in \mathcal{M}_{i't'} \\ 0 & \text{otherwise.} \end{cases} \quad (25)$$

Proof is in Appendix A.3. In this theorem, $w_{it}^{i't'}$ represents the amount of contribution or "matching weight" of observation (i, t) for the estimation of treatment effect of observation (i', t'). For any observation (i, t), its regression weight is given by the sum of the matching weights across all observations.

For example, it can be shown that when the matched set is given by Equation 13, the regression weights equal to the inverse of the propensity score computed within each unit. Thus, along with Proposition 2, this implies that if the data-generating process is given by the linear unit fixed effects model defined in Equation 1 with Assumptions 2 and 3, then the weighted linear unit fixed effects regression estimator with weights inversely proportional to the propensity score is consistent for the average treatment effect β. We note that this weighted linear fixed effects estimator is numerically equivalent to the sample weighted treatment effect estimator of Wooldridge (2005b), which was further studied by Gibbons, Suárez Serrato, and Urbancic (2017).

Theorem 1 yields several practically useful implications. First, one can efficiently compute within-unit matching estimators even when the number of units is large. Specifically, a weighted linear fixed effects estimator can be computed by first subtracting its within-unit weighted average from each of the variables and then running another weighted regression using these "weighted demeaned" variables. Second, taking advantage of this equivalence, we can use various model-based robust standard errors for within-unit matching estimators (e.g., Stock and Watson 2008; White 1980a). Third, a within-unit matching estimator is consistent for the ATE even when $\mathsf{LIN-FE}$ is the true model (i.e., the linearity assumption holds). This observation leads to a simple specification test based on the difference between the unweighted and weighted least squares (White 1980b) where the null hypothesis is that the linear unit fixed effects regression model is correct.

Finally, Theorem 1 can be used to improve the credibility of the BA design. Recall that the BA design makes the assumption that there is no time trend (Assumption 5). That is, the outcome from the previous time period can be used to estimate the average counterfactual outcome in the next time period when the treatment status changes. In practice, however, this identification assumption may be questionable, especially when estimating a long-term average treatment effect defined in Equation 21. One possibile way to address this problem is to exploit the equivalence between matching and weighted least squares estimators and model a time trend using observations prior to the administration of treatment.

For example, researchers may use the following weighted least squares estimator of the average treatment effect in the F time periods ahead,

$$\hat{\beta} = \underset{\beta}{\operatorname{argmin}} \sum_{i=1}^{N} \sum_{t=1}^{T} W_{it} \left\{ (Y_{it} - \overline{Y}_i) - \beta(X_{it} - \overline{X}_i) - f_\gamma(t) \right\}^2, \quad (26)$$

where $f_\gamma(t)$ is a parametric time trend model (e.g., $f_\gamma(t) = \gamma_1(t - \bar{t}) + \gamma_2(t^2 - \bar{t^2})$), with $\bar{t}$ and $\bar{t^2}$ representing the average year, and average squared year respectively,[12] and W_{it} is the weight computed according to Theorem 1 based on the matched set defined in Equation 20:

$$W_{it} = D_{it} \sum_{i'=1}^{N} \sum_{t'=1}^{T} w_{it}^{i't'} \quad \text{where}$$

$$w_{it}^{i't'} = \begin{cases} 1 & \text{if } (i,t) = (i', t'+F) \\ 1/|\mathcal{M}_{i't'}| & \text{if } (i,t) \in \mathcal{M}_{i't'} \\ 0 & \text{otherwise,} \end{cases} \quad (27)$$

and $\mathcal{M}_{it}$ is defined in Equation 20. Although the above model assumes a common time trend across units, we can also estimate a separate time trend for each unit if there is a sufficient number of time periods. This is done by replacing $f_\gamma(t)$ with $f_{\gamma_i}(t)$ (e.g., $f_{\gamma_i}(t) = \gamma_{i1}(t - \bar{t}) + \gamma_{i2}(t^2 - \bar{t^2})$). While this strategy enables flexible modeling of time trend, care must be taken especially for a large value of F since we are extrapolating into the future.

An Empirical Illustration

In this section, we illustrate our proposed methodology by estimating the effects of General Agreement on Tariffs and Trade (GATT) membership on bilateral trade and comparing our results with various fixed effects models. We show that different causal assumptions can yield substantively different results, but our methodology using the before-and-after design generally suggests that joint GATT membership slightly increases bilateral trade volume on average.

[12] For an unbalanced time-series cross section data set, $\bar{t}$ and $\bar{t^2}$ will vary across units.

Effects of GATT Membership on Bilateral Trade

Does GATT membership increase international trade? Rose (2004) finds that the answer to this question is negative. Based on the standard gravity model applied to dyadic trade data, his analysis yields economically and statistically insignificant effects of GATT membership (and its successor, the World Trade Organization or WTO) on bilateral trade. This finding led to subsequent debates among empirical researchers as to whether GATT actually promotes trade (e.g., Gowa and Kim 2005; Tomz, Goldstein, and Rivers 2007; Rose 2007). In particular, Tomz, Goldstein and Rivers (2007) find a substantial positive effect of GATT/WTO on trade when a broader definition of membership is employed. They argue that *nonmember participants*, such as former colonies, de facto members, and provisional members, should also be included in empirical analysis since they enjoy similar "rights and obligations."

Despite the substantive controversy regarding the effects of GATT on trade, researchers appear to have reached a methodological consensus that the $\mathsf{LIN}-\mathsf{FE}$ is the correct model to be used (Anderson and Wincoop 2003; Feenstra 2003). In another article, Rose (2005) expresses his belief in the usefulness of $\mathsf{LIN}-\mathsf{FE}$ by stating, "In terms of confidence, I follow the profession in placing most confidence in the fixed effects estimators; I have no clear ranking between country-specific and country pair-specific effects" (10). Tomz, Goldstein, and Rivers (2007) agree with this assessment and write, "We, too, prefer FE estimates over OLS on both theoretical and statistical grounds" (2013). Below, we critically examine the assumptions of the $\mathsf{LIN}-\mathsf{FE}$ in the context of this specific application.

Data

We analyze the data set from Tomz, Goldstein, and Rivers (2007), which updates and corrects some minor errors in the data set used by Rose (2004). Unlike Rose (2004) and Tomz, Goldstein, and Rivers (2007), however, we restrict our analysis to the period between 1948 and 1994 so that we focus on the effects of GATT and avoid conflating them with the effects of the WTO. As shown below, this restriction does not significantly change the conclusions of the studies, but it leads to a conceptually cleaner analysis. This yields a dyadic data set of bilateral international trade in which the total number of dyads is 10,289 and the total number of (dyad-year) observations is 196,207.

We use two different definitions of GATT membership: "formal membership" as used by Rose (2004) and "participants" as adopted by Tomz, Goldstein, and Rivers (2007). For each membership definition, we estimate its average effects on bilateral trade. We consider the two treatment variables: whether both countries in a dyad i are members (formal or participants) of GATT or not in a given year t (mix of dyads with one GATT member and no GATT member). This analysis focuses on the reciprocity hypothesis that GATT can impact bilateral trade only when countries mutually agree on reducing trade barriers (Bagwell and Staiger 1999).[13]

Figure 4 shows the distributions of these two treatment variables across dyads (vertical axis) and over time (horizontal axis). For any dyad, the treatment status changes at most once in only one direction from the control condition to the treatment condition. This observation holds true for both formal membership (left plot) and participant status (right plot). Given this distribution of treatment variables, we next consider different identification strategies.

Models and Assumptions

We begin with the following linear regression model with dyadic fixed effects used by Rose (2007):

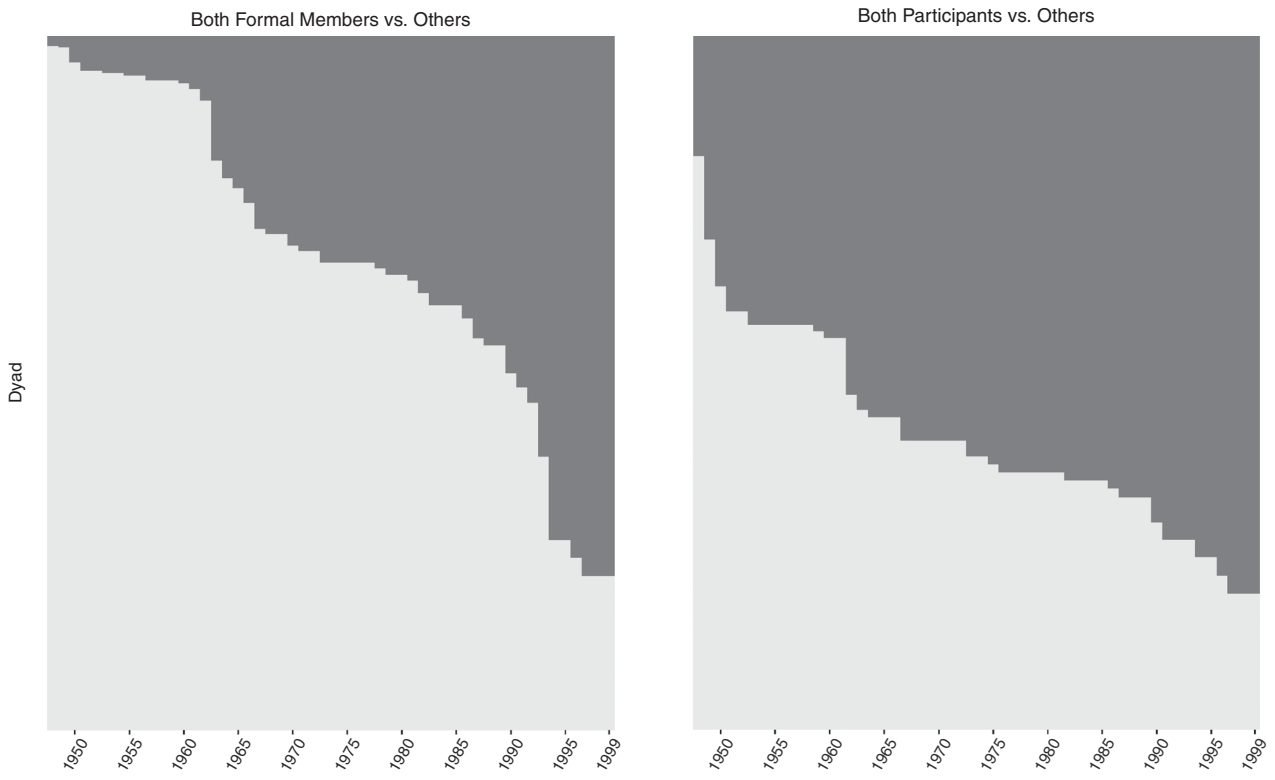
$$\log Y_{it} = \alpha_i + \beta X_{it} + \delta^\top \mathbf{Z}_{it} + \epsilon_{it}, \qquad (28)$$

where X_{it} is one of the binary treatment indicators for dyad i in year t described above, Y_{it} is the bilateral trade volume, and $\mathbf{Z}_{it}$ represents a vector of time-varying confounders including Generalized System of Preferences (GSP), log product real GDP, log product real GDP per capita, regional free trade agreement, currency union, and currently colonized. As discussed earlier, the advantage of this standard dyad fixed effects model is its ability to adjust for unobserved time-invariant confounders.

Next, we progressively improve this $\mathsf{LIN}-\mathsf{FE}$. First, we relax the linearity assumption, which, as shown in Proposition 1, leads to bias when there exists heterogeneity across dyads in treatment effect and/or treatment assignment probability. Indeed, Subramanian and Wei (2007) find substantial heterogeneity in the effects of GATT/WTO on trade. We instead apply the proposed nonparametric matching estimator, given by Equation 14, which compares each treated observation with the average outcome among *all* control observations within the same unit. In the current application, this implies the within-dyad comparison between the control observations in an

[13] We also conduct analyses based on alternative definitions of treatment, such as whether only one country or no country in a dyad is a member (see Appendix A.4).

FIGURE 4 Distributions of the Treatment Variables across Dyads and over Time



Note: This figure displays the distribution of treatment for 9,180 dyads from 1948 to 1999 based on 136 countries whose existence and membership status were identified by Tomz, Goldstein, and Rivers (2007) during the entire period. In the left (right) panel, the binary treatment variable is shown in dark gray if both countries in the dyad are formal members (participants) and in light gray otherwise. In both cases, the visualization shows that the treatment status does not revert back and forth over time because countries do not exit the institution once they join.

earlier period and the treatment observations in a later period because the treatment status changes at most once for any given dyad. However, this comparison is potentially problematic since the data set spans a relatively large number of years and hence some of the matched control observations may be too far apart in time from the treated observation to be comparable. For example, it would not be credible if one estimated the counterfactual bilateral trade volume in 1994 using the observed trade volume in 1950 since various other factors have changed between those 2 years.

Second, to improve the comparability of treated and control observations, we employ the first-difference estimator by restricting the matched set and compare the observations within only two subsequent time periods with treatment status change (see Equation 19). Under this special case of the before-and-after design, we require the assumption of no time trend because the control observation immediately before the change in treatment status is used to form an estimate of the counterfactual outcome under the treatment condition. Although this identification strategy is more credible, the assumption of no time trend may be too stringent given that trade volume in general has increased over time. This problem is particularly pronounced if researchers are interested in estimating the long-term effects (see Equation 21) rather than the contemporaneous effect.

Thus, we generalize the first-difference estimator by including longer lagged control observations in the matched set prior to the change in the treatment status (see Equation 20). This allows us to parametrically adjust for the time trend in bilateral trade volume by exploiting the equivalence between matching and weighted fixed effects estimators. Under this general design, we can also estimate both contemporaneous and longer-term effects by setting various values of leads, $F \geq 0$. Specifically, we fit the following weighted fixed effects estimator with a

TABLE 2 Estimated Contemporaneous Effects of GATT on the Logarithm of Bilateral Trade Based on Various Dyad Fixed Effects Estimators

	Dyad Fixed Effects			Before-and-After Design		
Membership	**Standard**	**Weighted**	**First Diff.**	**Lag = 3**	**Lag = 5**	**Lag = 7**
Formal	−0.048	−0.069	0.075	−0.003	0.028	0.035
(N=196,207)	(0.024)	(0.022)	(0.053)	(0.027)	(0.022)	(0.020)
White's p-value		0.000	0.004			
N (nonzero weights)	196,207	110,195	6,846	8,625	12,880	17,274
Participants	0.147	0.011	0.096	0.066	0.065	0.079
(N=196,207)	(0.030)	(0.028)	(0.029)	(0.035)	(0.028)	(0.026)
White's p-value		0.000	0.648			
N (nonzero weights)	196,207	68,004	3,952	4,472	6,640	8,905
Covariates	Year-varying dyadic covariates			Quadratic time trends		

Note: The "Standard" column presents the estimates based on the standard linear regression model with dyadic fixed effects given by Equation 28. The "Weighted" column presents the estimates based on the nonparametric matching estimator given by Equation 12. The "First Diff." column is based on the comparison between two subsequent time periods with treatment status change. "White's p-value" represents the p-value from a model specification test whose null hypothesis is that the standard linear fixed effects models are correct. Finally, the "Before-and-After Design" columns present the results based on three different lengths of lags with quadratic time trends (Equation 29). These estimators compare the dyads of two GATT members with those consisting of either one or no GATT member. "Formal" membership includes only formal GATT members as done in Rose (2004), whereas "Participants" includes nonmember participants as defined in Tomz, Goldstein, and Rivers (2007). The year-varying dyadic covariates include Generalized System of Preferences, log product real GDP, log product real GDP per capita, regional free trade agreement, currency union, and currently colonized. Robust standard errors, allowing for the presence of heteroskedasticity, are in parentheses. The results suggest that different causal assumptions, which imply different regression weights, can yield different results, and that the standard linear fixed effects models are likely to be misspecified.

quadratic time trend:

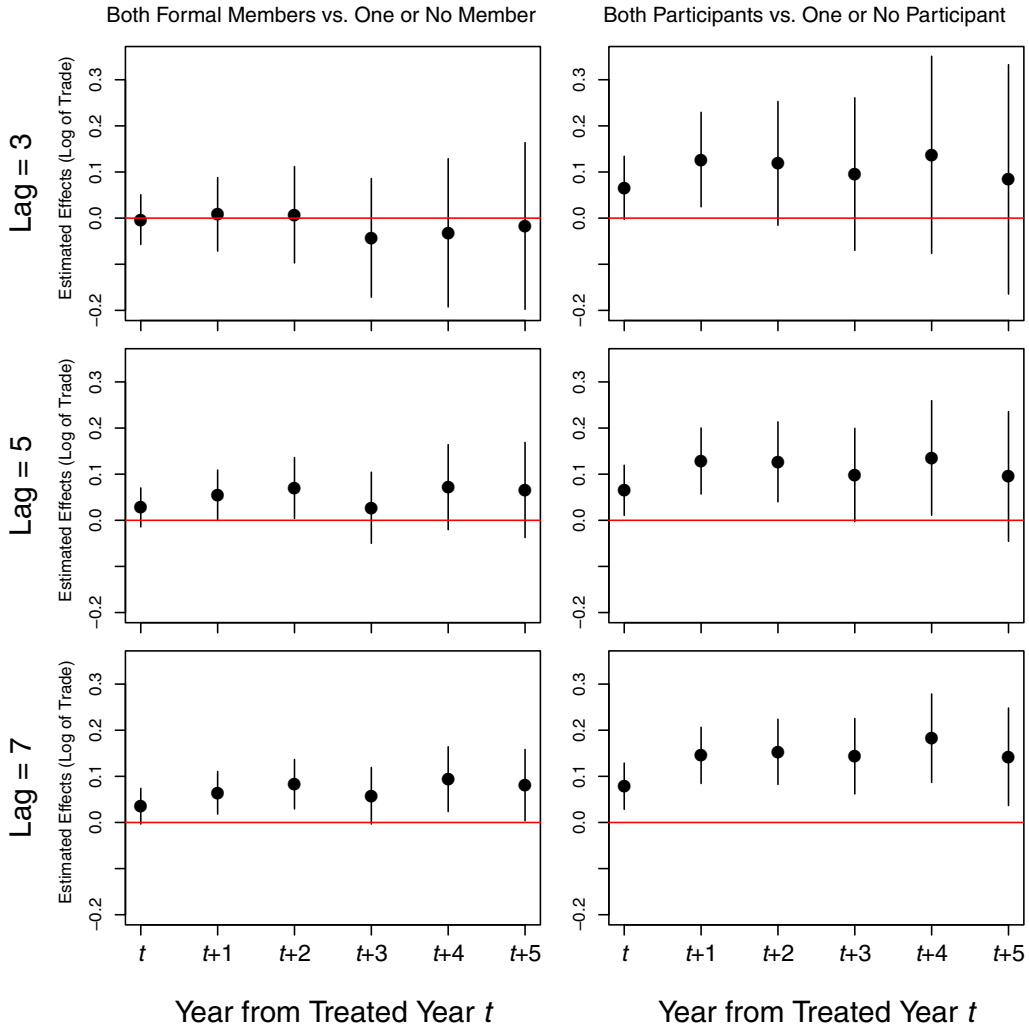
$$\hat{\beta}_{\mathrm{BA}} = \underset{\beta}{\operatorname{argmin}} \sum_{i=1}^{N} \sum_{t=1}^{T} W_{it} \left\{ (Y_{it} - \overline{Y}_i) - \beta(X_{it} - \overline{X}_i) - \gamma_1(t - \bar{t}_i) - \gamma_2(t^2 - \bar{t}_i^2) \right\}^2, \tag{29}$$

where W_{it} is given by Equation 25, and $\bar{t}_i$ and $\bar{t}_i^2$ are the mean values of year and squared year variables, respectively (unlike the estimator in Equation 26, these values may differ across dyads because some dyads do not cover the entire period in this application). In this application, we chose at most $F = 5$ as our maximum value of this lead variable since the identification of longer-term effects requires the extrapolation of time trends into the future based on the observed control observations from the past.

A word of caution is warranted about these estimators. As discussed earlier, these dyad linear fixed effects regression models and the before-and-after designs critically assume that there exist no time-varying unobserved confounders and that past outcomes do not confound the causal relationship between current treatment and outcome.[14] These assumptions may be unrealistic. Studies have shown that economic interests and previous levels of engagement in bilateral trade affect countries' incentives to join the GATT/WTO (Davis and Wilf 2017). That is, past outcomes (i.e., bilateral trade volumes) may affect current treatment (i.e., GATT membership). Furthermore, past treatments may also affect current outcome. Atkeson and Burstein (2010) find that changes in trade barriers, such as most favored nations (MFN) tariffs applied to GATT members, will determine forward-looking firms' reactions to exit, export, and product innovation, which will in turn affect future trade volumes. For the empirical analysis of this article, however, we maintain these assumptions and focus on the aforementioned improvements of the linear fixed effects regression estimator used in the literature. In the concluding section, we briefly discuss potential extensions of the proposed methodology to address these fundamental identification assumptions of unit fixed effects regression estimators.

[14] Moreover, the model assumes no interference between units. That is, one dyad's treatment status does not affect the trade volumes of other dyads. Although this assumption may also be unrealistic in this interdependent world, relaxing it is difficult and is beyond the scope of this article.

FIGURE 5 Estimated Longer-Term Effects of GATT on the Logarithm of Bilateral Trade Based on Before-and-After Design



Note: This plot presents point estimates and 95% confidence intervals for the estimated effects of GATT membership on bilateral trade. The quantity of interest and the matched set are given by Equations 21 and 20, respectively. That is, we compare the average bilateral trade volume across $L \in \{3, 5, 7\}$ years of lags before the treatment given at year t against the bilateral trade volume in $t + F$, where $F \in \{0, 1, 2, 3, 4, 5\}$ is the years since the treatment. We also include quadratic time trends. Overall, we find no evidence of positive effects of formal membership. We find some evidence of positive effects of participant membership with substantively much smaller effect sizes (e.g., 20% increase in 5 years) than the estimates from Tomz, Goldstein, and Rivers (2007; e.g., 71.6% contemporaneous effects). Robust standard errors allowing for the presence of heteroskedasticity are used.

Findings

We present the results based on each estimator discussed above. Table 2 summarizes the estimated contemporaneous effects of GATT membership on bilateral trade volume. When we use the standard linear regression model with dyadic fixed effects (see the "Standard" column), we find that formal membership does not increase trade volume on average (if anything, there appears to be a negative effect). In contrast, the estimated effect of participant is postive and statistically significant. As expected, these results are consistent with the original findings from

both Rose (2004) and Tomz, Goldstein, and Rivers (2007), who also used standard fixed effects regression models.

However, the estimates based on the nonparametric matching estimator allowing for heterogeneity in treatment assignment and treatment effects suggest that the effect of participant membership is much smaller and statistically indistinguishable from zero (see the "Weighted" column). The nonparametric matching estimator uses a much lower number of observations than the standard dyadic fixed effects because the dyads with no treatment status change are dropped. The standard dyadic fixed effects model still includes these observations because of year-varying dyadic covariates.

In contrast, the analysis based on the comparison between two subsequent time periods with treatment status change, corresponding to the "First Diff." column, shows that the estimated effects for both treatment variables are positive, although the effect of formal membership is not statistically significant. The sample size for this analysis is further reduced because of its focus on immediately before and after the change in treatment status. As a consequence, the standard error for the estimated effect of formal membership is substantially greater than those of standard and weighted analyses. On the other hand, the standard error for the estimated effect of participant does not increase as much, suggesting that the effect is relatively precisely estimated. "White p-value" represents the p-value from a model misspecification test whose null hypothesis is that the standard linear fixed effects models are correct (see the subsection "Estimation, Inference, and Specification Test"). The small p-values suggest that the standard linear fixed effects models are likely to be misspecified.

Furthermore, we implement the before-and-after design with three different lengths of lag to adjust for the time trends in trade volume for each dyad. Consistent with the above analyses, we find that the estimated contemporaneous effect of the formal membership is not distinguishable from zero with small point estimates, regardless of the lag length. However, while the standard error tends to be greater with fewer lags, we consistently find positive effects of participant membership across models with various lags. In sum, our findings suggest that different causal assumptions can yield different results. Credible comparisons under the before-and-after design yield a robust finding that the contemporaneous effect of participant is positive, whereas that of formal membership is not statistically distinguishable from zero.

Finally, we estimate the longer-term effects of the two treatment variables given in Equation 21. Figure 5 presents the point estimates and 95% confidence intervals for the estimated effects of formal membership (left column) and participant (right column) on trade volume from the year of treatment t to year $t + 5$. We also use various lengths of lag to examine the robustness of our findings. The quadratic time trend is included in the model to account for time trend in trade volume.

In general, we find little evidence for longer-term effects of formal membership. Notwithstanding the statistically significant positive effects estimated based on 7 years of lags, the effect sizes are modest: The highest estimate suggests an 8% percent ($\approx \exp(0.081) - 1$) increase in trade volume over 5 years after joining the GATT. Similarly, we find some evidence of positive effects of participant membership with the model with the lag of 7 years. However, the estimated effect sizes are substantively much smaller (e.g., maximum 20% increase in 5 years) than the estimates reported in Tomz, Goldstein, and Rivers (e.g., 71.6% contemporaneous effect).

We note that the estimated effect sizes are stable across various implementations of the before-and-after design with different values of the lags, although a longer lag yields smaller standard errors due to a larger sample size. To account for the correlations across dyads when they share a common member, we verify our findings with the cluster-robust standard errors proposed by Aronow, Samii, and Assenova (2015) and find that the positive effects of participant membership remain statistically significant, although the standard errors are about 20% larger on average. Our findings are also robust to including/excluding time-varying covariates as well as linear time trend (see Appendix A.5 for additional results). This is not surprising since matching is expected to reduce model dependence (Ho et al. 2007).

Concluding Remarks

The title of this article asks the question of when researchers should use linear unit fixed effects regression models for causal inference with longitudinal data. According to our analysis, the answer to this question depends on the trade-off between unobserved time-invariant confounders and dynamic causal relationships between outcome and treatment variables. In particular, if the treatment assignment mechanism critically depends on past outcomes, then researchers are likely to be better off investing their efforts in measuring and adjusting for time-varying confounders rather than adjusting for unobserved time-invariant confounders through unit fixed

effects models under unrealistic assumptions. In such situations, methods that effectively account for dynamics such as marginal structural models may be more appropriate. This conclusion also applies to the before-and-after designs that are closely related to first-differencing regression models.

If, on the other hand, researchers are concerned about time-invariant confounders and are willing to assume the absence of dynamic causal relationships, then unit fixed effects regression models are effective tools to adjust for unobserved time-invariant confounders. In this article, we propose a new matching framework that improves linear unit fixed effects regression models by relaxing the linearity assumption. Under this framework, we show how to incorporate various identification strategies and implement them as weighted linear unit fixed effects regression estimators. For example, we extend the first-difference estimator to the general case of the before-and-after design and show its equivalence to a weighted linear regression with unit fixed effects. Our framework also facilitates the incorporation of additional covariates, model-based inference, and specification tests.

Unfortunately, researchers must choose either to adjust for unobserved time-invariant confounders through unit fixed effects models or to model dynamic causal relationships between treatment and outcome under a selection-on-observables approach. No existing method can achieve both objectives without additional assumptions. In addition, because causal inference with observational data always requires unverifiable assumptions, there is no statistical test that can tell researchers which method is more appropriate. A primary goal of this article is to clarify the required assumptions of unit fixed effects models, which are widely used in social sciences. While we limit our discussion to the case of a binary treatment, our nonparametric identification analysis based on DAGs is applicable to the case in which the treatment is nonbinary. Thus, researchers who are analyzing a nonbinary treatment must face the same trade-off described here.

In this article, we show that causal inference with unit fixed effects regression models is fundamentally based on the within-unit comparison between the treated and control observations. The major limitation of this identification strategy is that one must assume a certain within-unit time trend for the average potential outcome. We used past control observations to model this time trend, but an alternative strategy is to use the observations from other similar units to estimate unit-specific time trend. Due to space limitations, we do not explore such an approach, which is closely related to two-way fixed effects models. Imai, Kim, and Wang (2018) extend our matching framework introduced here and generalize the difference-in-differences identification strategy.

Appendix A: Mathematical Appendix

A.1 Equivalence between Assumptions 1 and 3

Consider the potential outcome model $Y_{it}(x) = g(x, \mathbf{U}_i, \epsilon_{it})$, which is consistent with equation 5. It is obvious that under this model, Equation 5 of Assumption 1 implies Assumption 3. To prove the converse, we focus on the case with $T = 3$, as the same argument can be repeatedly applied to the case with $T > 3$.

$$
\begin{aligned}
&p(\{Y_{it}(1),\, Y_{it}(0)\}_{t=1}^{3},\, X_{i1},\, X_{i2},\, X_{i3} \mid \mathbf{U}_i) \\
&\quad = p(\{Y_{it}(1),\, Y_{it}(0)\}_{t=1}^{3} \mid X_{i1},\, X_{i2},\, X_{i3},\, \mathbf{U}_i) \\
&\qquad p(X_{i1},\, X_{i2},\, X_{i3} \mid \mathbf{U}_i) \\
&\quad = p(\{Y_{it}(1),\, Y_{it}(0)\}_{t=1}^{3} \mid X_{i1},\, X_{i2},\, \mathbf{U}_i) \\
&\qquad p(X_{i1},\, X_{i2},\, X_{i3} \mid \mathbf{U}_i) \\
&\quad = p(\{Y_{it}(1),\, Y_{it}(0)\}_{t=1}^{3} \mid X_{i1},\, \mathbf{U}_i) \\
&\qquad p(X_{i1},\, X_{i2},\, X_{i3} \mid \mathbf{U}_i) \\
&\quad = p(\{Y_{it}(1),\, Y_{it}(0)\}_{t=1}^{3} \mid \mathbf{U}_i) \\
&\qquad p(X_{i1},\, X_{i2},\, X_{i3} \mid \mathbf{U}_i),
\end{aligned}
$$

which shows that $\{Y_{it}(1),\, Y_{it}(0)\}_{t=1}^{3}$ are conditionally independent of $\mathbf{X}_i$ given $\mathbf{U}_i$.

A.2 Proof of Proposition 2

We begin by rewriting the within-unit matching estimator as

$$
\hat{\tau}_{\mathsf{match}} = \frac{1}{\frac{1}{N}\sum_{i=1}^{N} C_i} \cdot \frac{1}{N} \sum_{i=1}^{N} C_i \left(\frac{\sum_{t=1}^{T} X_{it} Y_{it}}{\sum_{t=1}^{T} X_{it}} - \frac{\sum_{t=1}^{T} (1 - X_{it}) Y_{it}}{\sum_{t=1}^{T} (1 - X_{it})} \right). \tag{30}
$$

By law of large numbers, the first term converges in probability to $1/\Pr(C_i = 1)$. To derive the limit of the second term, first note that Assumption 3 implies the following conditional independence:

$$
\{Y_{it}(1),\, Y_{it}(0)\} \perp\!\!\!\perp \mathbf{X}_i \mid \mathbf{U}_i. \tag{31}
$$

The law of iterated expectation implies

$$\mathbb{E}(Y_{it}(x) \mid C_i = 1) = \mathbb{E}\{\mathbb{E}(Y_{it}(x) \mid \mathbf{U}_i, C_i = 1) \mid C_i = 1\} \tag{32}$$

for $x = 0, 1$. We show that the difference of means over time within unit i estimates the inner expectation of Equation 32 without bias because it adjusts for $\mathbf{U}_i$. Consider the case with $x = 1$.

$$\begin{aligned}
&\mathbb{E}\left(C_i \frac{\sum_{t=1}^T X_{it} Y_{it}}{\sum_{t=1}^T X_{it}}\right) \\
&= \mathbb{E}\left\{\frac{1}{\sum_{t=1}^T X_{it}} \sum_{t=1}^T X_{it} \mathbb{E}(Y_{it}(1) \mid \mathbf{X}_i, \mathbf{U}_i) \,\middle|\, C_i = 1\right\} \Pr(C_i = 1) \\
&= \mathbb{E}\left\{\frac{1}{\sum_{t=1}^T X_{it}} \sum_{t=1}^T X_{it} \mathbb{E}(Y_{it}(1) \mid C_i = 1, \mathbf{U}_i) \,\middle|\, C_i = 1\right\} \Pr(C_i = 1) \\
&= \mathbb{E}(Y_{it}(1) \mid C_i = 1) \Pr(C_i = 1),
\end{aligned}$$

where the second equality follows from Equation 31. We note that $\hat{\tau}_{\mathsf{match}}$ can be more precisely defined as $1/|\mathcal{I}| \sum_{i \in \mathcal{I}} \{\sum_{t=1}^T X_{it} Y_{it} / \sum_{t=1}^T X_{it} - \sum_{t=1}^T (1 - X_{it}) Y_{it} / \sum_{t=1}^T (1 - X_{it})\}$ where $\mathcal{I} := \{i \in \{1, \ldots, N\} \mid C_i = 1\}$ to avoid the division by zero. A similar argument can be made to show $\mathbb{E}\left(C_i \frac{\sum_{t=1}^T (1 - X_{it}) Y_{it}}{\sum_{t=1}^T (1 - X_{it})}\right) = \mathbb{E}(Y_{it}(0) \mid C_i = 1) \Pr(C_i = 1)$. Thus, the second term of Equation 30 converges to $\mathbb{E}(Y_{it}(1) - Y_{it}(0) \mid C_i = 1) \Pr(C_i = 1)$. The result then follows from the continuous mapping theorem. □

A.3 Proof of Theorem 1

We begin this proof by establishing two algebraic equalities. First, we prove that for any constants $(\alpha_1^*, \ldots, \alpha_N^*)$, the following equality holds:

$$\begin{aligned}
&\sum_{i=1}^N \sum_{t=1}^T W_{it}(2X_{it} - 1)\alpha_i^* \\
&= \sum_{i'=1}^N \sum_{t'=1}^T \left(\sum_{i=1}^N \sum_{t=1}^T D_{it} w_{it}^{i't'} (2X_{it} - 1)\alpha_i^*\right) \\
&= \sum_{i'=1}^N \sum_{t'=1}^T \left(\sum_{i=1}^N \sum_{t=1}^T D_{it} \left\{X_{i't'} w_{it}^{i't'} (2X_{it} - 1)\alpha_i^* \right.\right. \\
&\qquad \left.\left. + (1 - X_{i't'}) w_{it}^{i't'} (2X_{it} - 1)\alpha_i^* \right\}\right) \\
&= \sum_{i'=1}^N \sum_{t'=1}^T D_{it} \left\{ X_{i't'} \left(\alpha_i^* - \sum_{(i,t) \in \mathcal{M}_{i't'}} \frac{1}{|\mathcal{M}_{i't'}|} (1 - X_{it}) \alpha_i^*\right) \right. \\
&\qquad \left. + (1 - X_{i't'}) \left(\sum_{(i,t) \in \mathcal{M}_{i',t'}} \frac{1}{|\mathcal{M}_{i't'}|} X_{it} \alpha_i^* - \alpha_i^*\right)\right\} \\
&= \sum_{i'=1}^N \sum_{t'=1}^T D_{it} \left\{ X_{i't'} (\alpha_i^* - \alpha_i^*) + (1 - X_{i't'})(\alpha_i^* - \alpha_i^*) \right\} = 0,
\end{aligned} \tag{33}$$

where the last equality follows from $\sum_{(i,t) \in \mathcal{M}_{i't'}} \frac{1}{|\mathcal{M}_{i't'}|} (1 - X_{it}) = 1$ if $X_{i't'} = 1$, and $\sum_{(i,t) \in \mathcal{M}_{i't'}} \frac{1}{|\mathcal{M}_{i't'}|} X_{it} = 1$ if $X_{i't'} = 0$.

Similarly, the second algebraic equality we prove is the following:

$$\begin{aligned}
&\sum_{i=1}^N \sum_{t=1}^T W_{it} \\
&= \sum_{i=1}^N \sum_{t=1}^T D_{it} \left(\sum_{i'=1}^N \sum_{t'=1}^T w_{it}^{i't'}\right) \\
&= \sum_{i'=1}^N \sum_{t'=1}^T D_{it} \left(\sum_{i=1}^N \sum_{t=1}^T w_{it}^{i't'}\right) \\
&= \sum_{i'=1}^N \sum_{t'=1}^T D_{it} \left(\sum_{i=1}^N \sum_{t=1}^T X_{i't'} w_{it}^{i't'} + (1 - X_{i't'}) w_{it}^{i't'}\right) \\
&= \sum_{i'=1}^N \sum_{t'=1}^T D_{it} \left[X_{i't'} \left(1 + \sum_{(i,t) \in \mathcal{M}_{i't'}} \frac{1}{|\mathcal{M}_{i't'}|} (1 - X_{it})\right) \right. \\
&\qquad \left. + (1 - X_{i't'}) \left(1 + \sum_{(i,t) \in \mathcal{M}_{i't'}} \frac{1}{|\mathcal{M}_{i't'}|} X_{it}\right)\right] \\
&= \sum_{i'=1}^N \sum_{t'=1}^T D_{it} \{X_{i't'}(1 + 1) + (1 - X_{i't'})(1 + 1)\} \\
&= 2 \sum_{i=1}^N \sum_{t=1}^T D_{it}.
\end{aligned} \tag{34}$$

Third, we show that $\overline{X}_i^* = 1/2$.

$$\begin{aligned}
\overline{X}_i^* &= \frac{\sum_{t=1}^T W_{it} X_{it}}{\sum_{t=1}^T W_{it}} \\
&= \frac{\sum_{t=1}^T D_{it} \sum_{i'=1}^N \sum_{t'=1}^T w_{it}^{i't'} X_{it}}{\sum_{t=1}^T D_{it} \sum_{i'=1}^N \sum_{t'=1}^T w_{it}^{i't'}} \\
&= \frac{\sum_{t=1}^T D_{it} \sum_{i'=1}^N \sum_{t'=1}^T \left(X_{i't'} w_{it}^{i't'} X_{it} + (1 - X_{i't'}) w_{it}^{i't'} X_{it}\right)}{\sum_{t=1}^T D_{it} \sum_{i'=1}^N \sum_{t'=1}^T \left(X_{i't'} w_{it}^{i't'} + (1 - X_{i't'}) w_{it}^{i't'}\right)} \\
&= \frac{\sum_{t=1}^T D_{it} \cdot (1 + 0)}{\sum_{t=1}^T D_{it} \cdot (1 + 1)} \\
&= \frac{1}{2},
\end{aligned}$$

where the fourth equality follows from the fact that (1) $w_{it}^{i't'} = 1$ when $X_{it} = X_{i't'}$ and 0 otherwise, and (2) $(1 - X_{i't'}) X_{it} = 0$ if $(i, t) \in \mathcal{M}_{i't'}$ because only the years with

opposite treatment status are in the matched set. This implies

$$X_{it} - \overline{X}_i^* = \begin{cases} \frac{1}{2} & \text{if } X_{it} = 1 \\ -\frac{1}{2} & \text{if } X_{it} = 0 \end{cases}. \tag{35}$$

Using the above algebraic equalities, we can derive the desired result.

$$\begin{aligned}
\hat{\beta}_{\mathsf{WFE}} &= \frac{\sum_{i=1}^N \sum_{t=1}^T W_{it}(X_{it} - \overline{X}_i^*)(Y_{it} - \overline{Y}_i^*)}{\sum_{i=1}^N \sum_{t=1}^T W_{it}(X_{it} - \overline{X}_i^*)^2} \\
&= \frac{2}{\sum_{i=1}^N \sum_{t=1}^T D_{it}} \sum_{i=1}^N \sum_{t=1}^T W_{it}(X_{it} - \frac{1}{2})(Y_{it} - \overline{Y}_i^*) \\
&= \frac{1}{\sum_{i=1}^N \sum_{t=1}^T D_{it}} \sum_{i=1}^N \sum_{t=1}^T \\
&\quad \left\{ W_{it}(2X_{it} - 1)Y_{it} - W_{it}(2X_{it} - 1)\overline{Y}_i^* \right\} \\
&= \frac{1}{\sum_{i=1}^N \sum_{t=1}^T D_{it}} \sum_{i=1}^N \sum_{t=1}^T W_{it}(2X_{it} - 1)Y_{it} \\
&= \frac{1}{\sum_{i=1}^N \sum_{t=1}^T D_{it}} \sum_{i=1}^N \sum_{t=1}^T \\
&\quad \left\{ D_{it}\left(\sum_{i'=1}^N \sum_{t'=1}^T w_{it}^{i't'} \right)(2X_{it} - 1)Y_{it} \right\} \\
&= \frac{1}{\sum_{i=1}^N \sum_{t=1}^T D_{it}} \sum_{i'=1}^N \sum_{t'=1}^T \\
&\quad \left[X_{i't'} D_{it}\left(\sum_{i=1}^N \sum_{t=1}^T w_{it}^{i't'}(2X_{it} - 1)Y_{it} \right) \right. \\
&\quad \left. + (1 - X_{i't'}) D_{it}\left(\sum_{i=1}^N \sum_{t=1}^T w_{it}^{i't'}(2X_{it} - 1)Y_{it} \right) \right] \\
&= \frac{1}{\sum_{i=1}^N \sum_{t=1}^T D_{it}} \sum_{i'=1}^N \sum_{t'=1}^T \\
&\quad \left[X_{i't'} \left(D_{it}Y_{it} - \sum_{(i,t) \in \mathcal{M}_{i't'}} \frac{D_{it}}{|\mathcal{M}_{i't'}|}(1 - X_{it})Y_{it} \right) \right. \\
&\quad \left. + (1 - X_{i't'}) \left(\sum_{(i,t) \in \mathcal{M}_{i't'}} \frac{D_{it}}{|\mathcal{M}_{i't'}|} | X_{it} - D_{it}Y_{it} \right) \right] \\
&= \frac{1}{\sum_{i=1}^N \sum_{t=1}^T D_{it}} \sum_{i=1}^N \sum_{t=1}^T D_{it}(\widehat{Y_{it}(1)} - \widehat{Y_{it}(0)}) = \hat{\tau},
\end{aligned}$$

where the second equality follows from Equation 34 and 35, and the fourth equality from Equation 33. The last equality follows from applying the definition of $\widehat{Y_{it}(1)}$ and $\widehat{Y_{it}(0)}$ given in Equation 15. □

A.4 Estimated Contemporaneous Effects of GATT Membership on Bilateral Trade with Alternative Membership Definitions

TABLE A1 Estimated Contemporaneous Effects of GATT on the Logarithm of Bilateral Trade Based on Alternative Membership Definitions

		Dyad Fixed Effects		
Comparison	**Membership**	**Standard**	**Weighted**	**First Diff.**
Both vs. One	Formal	−0.034	−0.061	0.076
	(N = 175,814)	(0.024)	(0.022)	(0.054)
	White's p-value		0.000	0.007
	N (nonzero weights)	175,814	100,055	6,712
	Participants	0.161	0.020	0.099
	(N = 187,651)	(0.030)	(0.029)	(0.030)
	White's p-value		0.000	0.613
	N (nonzero weights)	187,651	64,152	3,900
One vs. None	Formal	−0.011	−0.094	0.031
	(N = 109,702)	(0.039)	(0.039)	(0.065)
	White's p-value		0.000	0.000
	N (nonzero weights)	109,702	36,115	2,670
	Participants	0.179	−0.034	0.053
	(N = 70,298)	(0.060)	(0.056)	(0.061)
	White's p-value		0.000	0.000
	N (nonzero weights)	70,298	15,766	1,087
Covariates			Year-varying covariates	

Note: The "Weighted" column presents the estimates based on the nonparametric matching estimator given by Equation 12. "First Diff." is based on the comparison between two subsequent time periods with treatment status change. "Both vs. One" represents the comparison between dyads of two GATT members and those consisting of only one GATT member. "One vs. None" refers to the comparison between dyads consisting of only one GATT member and those of two non-GATT members. "Formal" membership includes only formal GATT members as done in Rose (2004), whereas "Participants" includes nonmember participants as defined in Tomz, Goldstein, and Rivers (2007). The covariates include Generalized System of Preferences, log product real GDP, log product real GDP per capita, regional free trade agreement, currency union, and currently colonized. "White's p-value" is based on the specification test with the null hypothesis that the corresponding standard fixed effects model is correct. Robust standard errors, allowing for the presence of serial correlation as well as heteroskedasticity (Arellano 1987; Hansen 2007), are in parentheses. The results suggest that different causal assumptions, which imply different regression weights, can yield different results.

A.5 Before-and-After Design: Effects of GATT Membership on Trade

TABLE A2 Estimated Effects of GATT Membership on the Logarithm of Bilateral Trade Based on Before-and-After Design

Before	**After**	**Both vs. Mix Formal Membership**				**Both vs. Mix Participants**			
Lag = 3	t	−0.003	−0.003	0.008	0.006	0.066	0.066	0.071	0.071
		(0.027)	(0.028)	(0.027)	(0.027)	(0.035)	(0.035)	(0.035)	(0.035)
	$t+1$	0.008	0.008	0.021	0.015	0.127	0.127	0.128	0.120
		(0.041)	(0.041)	(0.040)	(0.040)	(0.052)	(0.052)	(0.052)	(0.052)
	$t+2$	0.005	0.007	0.013	0.005	0.115	0.119	0.112	0.103
		(0.053)	(0.053)	(0.053)	(0.053)	(0.068)	(0.068)	(0.068)	(0.069)
	$t+3$	−0.049	−0.043	−0.036	−0.045	0.095	0.096	0.076	0.054
		(0.065)	(0.066)	(0.065)	(0.065)	(0.084)	(0.084)	(0.084)	(0.084)

(*Continued*)

TABLE A2 Continued

Before	After	Both vs. Mix Formal Membership				Both vs. Mix Participants			
	$t+4$	−0.040	−0.032	−0.021	−0.022	0.141	0.137	0.153	0.137
		(0.082)	(0.082)	(0.082)	(0.082)	(0.109)	(0.109)	(0.108)	(0.109)
	$t+5$	−0.043	−0.017	−0.013	−0.004	0.087	0.084	0.095	0.078
		(0.092)	(0.092)	(0.092)	(0.092)	(0.126)	(0.127)	(0.127)	(0.126)
Lag = 5	t	0.028	0.028	0.037	0.006	0.066	0.065	0.072	0.071
		(0.022)	(0.022)	(0.021)	(0.027)	(0.028)	(0.028)	(0.028)	(0.035)
	$t+1$	0.052	0.054	0.060	0.015	0.129	0.128	0.123	0.120
		(0.028)	(0.028)	(0.028)	(0.040)	(0.036)	(0.037)	(0.036)	(0.052)
	$t+2$	0.065	0.070	0.066	0.005	0.123	0.127	0.109	0.103
		(0.034)	(0.034)	(0.033)	(0.053)	(0.044)	(0.044)	(0.044)	(0.069)
	$t+3$	0.017	0.027	0.025	−0.045	0.096	0.098	0.076	0.054
		(0.039)	(0.039)	(0.039)	(0.065)	(0.051)	(0.052)	(0.051)	(0.084)
	$t+4$	0.057	0.072	0.079	−0.022	0.136	0.135	0.161	0.137
		(0.047)	(0.047)	(0.047)	(0.082)	(0.063)	(0.063)	(0.063)	(0.109)
	$t+5$	0.030	0.066	0.059	−0.004	0.097	0.095	0.118	0.078
		(0.052)	(0.053)	(0.052)	(0.092)	(0.071)	(0.072)	(0.071)	(0.126)
Lag = 7	t	0.033	0.035	0.049	0.006	0.079	0.079	0.088	0.071
		(0.020)	(0.020)	(0.020)	(0.027)	(0.026)	(0.026)	(0.025)	(0.035)
	$t+1$	0.059	0.064	0.077	0.015	0.145	0.145	0.145	0.120
		(0.024)	(0.024)	(0.023)	(0.040)	(0.031)	(0.031)	(0.031)	(0.052)
	$t+2$	0.073	0.083	0.085	0.005	0.148	0.153	0.142	0.103
		(0.027)	(0.027)	(0.027)	(0.053)	(0.036)	(0.036)	(0.036)	(0.069)
	$t+3$	0.042	0.058	0.063	−0.045	0.141	0.144	0.132	0.054
		(0.031)	(0.031)	(0.031)	(0.065)	(0.041)	(0.042)	(0.041)	(0.084)
	$t+4$	0.073	0.094	0.107	−0.022	0.182	0.183	0.212	0.137
		(0.036)	(0.036)	(0.036)	(0.082)	(0.049)	(0.049)	(0.048)	(0.109)
	$t+5$	0.037	0.081	0.077	−0.004	0.145	0.142	0.172	0.078
		(0.039)	(0.039)	(0.039)	(0.092)	(0.053)	(0.054)	(0.053)	(0.126)
Linear time trend		√	√	√	√	√	√	√	√
Quadratic time trend			√		√		√		√
Year-varying covariates				√	√			√	√

Note: This table summarizes the estimated effects of GATT membership on bilateral trade, that is, the comparison between dyads of two GATT members and those consisting of only one GATT member. To implement the before-and-after design, we compare the average bilateral trade volume across lags $= L \in \{3, 5, 7\}$ years against the bilateral trade volume in leads $= F \in \{0, 1, 2, 3, 4, 5\}$ years since the treated year, which yields the quantity of interest given in Equation 22. We find no evidence of short-term and long-term effects of formal membership. Although we find some evidence of long-term positive effects of participant membership with the model with Lag = 7, the estimated effects are about 2.6 to 7.8 times smaller than the estimate (0.56) from (Tomz, Goldstein, and Rivers 2007, p. 2012) Our findings are robust to including linear/quadratic time trends and with/without year-varying covariates. Robust standard errors allowing for the presence of heteroskedasticity are used.

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Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Table 3: Estimated Contemporaneous Effects of GATT on the Logarithm of Bilateral Trade based on Alternative Membership Definitions.

Table 4: Estimated Effects of GATT Membership on the Logarithm of Bilateral Trade based on Before-and-After Design.