Solving the Unit Root Problem in Causal Inference

October 21, 2025

Abstract

Because political processes evolve over time, estimating dynamic causal effects is of great interest to social scientists. But despite a clear goal for political scientists, two largely separate research agendas have come to occupy scholarly attention: how to derive causal inferences using time series and time series cross-sectional data, and how to address the challenge that integration in a variable's history poses for estimating dynamic regression models. Little scholarship sits at the intersection of these literature bases, even though unit roots pose a problem to identification for virtually all research designs that leverage temporal variation in data. We propose a unifying framework that bridges the conversation through a new dynamic causal estimand: the Generalized Dynamic Treatment Effect. We demonstrate that this causal estimand allows for inferences in levels even if a research design requires variables to be differenced, thus easily allowing for causal inferences even in the presence of integration. While our results can be incorporated into any research design, we demonstrate how they can be used to better interpret dynamic regression models through an empirical application on media responsiveness to the economy.

1 Introduction

Two separate research agendas have occupied scholarly attention concerning the analysis of time series and time series cross-sectional data in political science over the past decade. The first agenda seeks to apply to such data the potential outcomes framework that has come to dominate philosophical interpretation of quantitative social science. In general, causal inference under the potential outcomes framework requires three steps: the definition of a *theoretical estimand* of interest as a comparison between two or more potential outcomes, an identification of an *empirical estimand* that is a function of observable data that equals the theoretical estimand given a set of assumptions about the data-generating process, and the selection of a *statistical estimator* with desirable properties and minimal additional assumptions (Keller and Branson, 2024). Multiple theoretical estimands for dynamic causal inference have been defined, and identification analysis has detailed how scholars can learn about theoretical estimands from empirical estimands calculated from observable data (Blackwell and Glynn, 2018; Imai, Kim and Wang, 2023). And for both experimental and observational data, scholars have begun suggesting estimators with minimal additional assumptions beyond those used for identification (Blackwell, 2013; Abadie, Diamond and Hainmueller, 2015; Moraffah et al., 2021; Chiu et al., 2025).

A second agenda has come to focus on the role of integration and equation balance in the estimation of dynamic regression models (Grant and Lebo, 2016; Pickup and Kellstedt, 2023). Practitioners of such models have long known how to calculate dynamic quantities of interest from autoregressive distributed lag (ADL) models, which have a causal interpretation under certain assumptions (Box and Jenkins, 1976; Engle, Hendry and Richard, 1983). But estimating such a model with integrated time series data risks inconsistent estimates and spurious inferences (Granger and Newbold, 1974). Alternative estimators are available: the general error correction model (GECM) (a reparameterization of the ADL) is a valid

estimator whenever the variables are either stationary or cointegrated (De Boef and Keele, 2008). Unfortunately, some political scientists mistakenly assumed that the GECM was valid for variables that were integrated but not cointegrated, which resulted in problematic inferences across the discipline (Grant and Lebo, 2016).[1] The cumulative advice of this scholarship is that dynamic linear models must be balanced—the covariates on the right-hand side of the model are collectively of the same order of integration as the outcome variable—and, in the absence of cointegration, integrated variables should either be excluded from the analysis or differenced until stationary (Pickup and Kellstedt, 2023).

The intersection of these two agendas is not well understood but of immense importance. The consequences of stationarity for causal inference is rarely discussed in the discipline. But recent scholarly advances have demonstrated that integration is of profound importance to most research designs leveraging temporal variation. For some, like differences-in-differences, stationarity provides an alternative means of identification that allows for a form of doubly-robust estimation (Marx, Tamer and Tang, 2024). For other research designs, like synthetic control methods, a dependent variable that is integrated but not cointegrated causes inconsistent estimates and spurious results in the same way that it does for dynamic regression models (Masini and Medeiros, 2022). It is increasingly clear that integration can no longer be ignored: *all* scholars studying data over time must consider how integration affects their preferred research design and how to address the possibility of integration in their data.

In the context of dynamic regression models, integration muddles the interpretation of empirical estimands. Though the "solution" to integration—first-differencing—is easy to implement, the resulting estimator only generates empirical estimands for differenced variables. It is unclear how to calculate dynamic causal estimands for the undifferenced version of those variables—or said differently, for those variables in levels. And virtually all scholars would

[1]The scholarship that resulted from this problematic interpretation has been cataloged in multiple symposia published in political science journals: see Volume 10, Issue 4 of *Political Science Research & Methods* (2022), as well as Volume 24, Issue 1 of *Political Analysis* (2016).

like the ability to make inferences about the relationships between their variables in levels, even if statistical estimation requires using variables in differences. As it stands, there is no framework for making inferences about variables in levels when estimators use variables in differences.

We solve these problems by bringing the best advice of each approach into conversation. We begin by reviewing three essential dynamic causal estimands, both theoretically and empirically, before considering a set of assumptions that can be used to identify them. We then explicate more fully the problem of integration and unit roots for research designs using time series and time series cross-sectional data. We offer a solution to this problem through the Generalized Dynamic Treatment Effect: a new dynamic causal estimand that allows for a variety of common treatment histories and, more importantly, allows all scholars to make inferences in levels from research designs estimated in differences. Specifically, we apply this empirical estimand to the workhorse dynamic linear models: the ADL and the GECM. Finally, we demonstrate how these insights can be applied with dynamic linear models via an empirical example concerning media responsiveness to the economy (Soroka, 2006).

Through this work, we make a number of contributions to the literature. We generalize dynamic causal estimands to reflect the variety of scholastic goals in the literature and provide guidance for selecting among dynamic causal estimands for a particular independent variable. We prove how differenced variables can be used to make dynamic inferences about the same variables in levels that can be applied regardless of research design. We demonstrate that, when using the dynamic linear model, dynamic causal estimands for differenced variables are identical to dynamic causal estimands for those variables in levels if the dependent and independent variable are differenced the same number of times. Finally, we implement our techniques in an `R` package, `tseffects`.

2 Defining Dynamic Causal Estimands

There are a wide variety of research designs to choose from when making causal inferences using temporal variation in observational data (Moraffah et al., 2021). When analyzing time series data for a single unit, analysts can use dynamic regression models (De Boef and Keele, 2008; Linn, Lebo and Webb, 2025), structural vector dynamic regression models (Lütkepohl, 2013), structural nested mean models (Blackwell and Glynn, 2018), intervention analysis (Box and Tiao, 1975; Menchetti, Cipollini and Mealli, 2023), and marginal structural models (Blackwell, 2013). When analyzing time series cross-sectional data, scholars can use dynamic panel regression models (Beck and Katz, 2011), difference-in-differences (Chiu et al., 2025), and synthetic control methods (Abadie, Diamond and Hainmueller, 2015). And for both types of data, nonparameteric estimators exist for experiments (Bojinov and Shephard, 2019; Bojinov, Rambachan and Shephard, 2021; Bojinov, Simchi-Levi and Zhao, 2023). Each of these methodologies rely on unique identifying assumptions which may be more or less plausible in a particular context.

Though the details of each research design vary, they all have one big idea in common: they utilize temporal variation in data to make inferences about *dynamic causal estimands*: the effect that a change in the independent variable in the present has on the dependent variable in the future. For example, a dynamic causal estimand would describe the effect that an economic crisis has on government approval in the weeks, months, or even years after the crisis occurred. In contrast, a static causal estimand would describe the instantaneous effect that a change in the independent variable in the present has on the dependent variable in the present, such as the effect an economic crisis has on simultaneous government approval. While the interest in dynamic causal estimands has grown in recent years, most of the attention has been restricted to dynamic causal estimands for binary independent variables (Abadie, Diamond and Hainmueller, 2015; Blackwell and Glynn, 2018; Bojinov

and Shephard, 2019; Menchetti, Cipollini and Mealli, 2023; Bojinov, Simchi-Levi and Zhao, 2023; Chiu et al., 2025). Binary variables can represent a broad range of political phenomena, as many decisions have a discrete numbers of alternatives that can be measured in a binary fashion. But there are also many phenomena that are continuous in nature, such as financial transactions and aggregate public opinion.

We advance our understanding of dynamic causal estimands for continuous independent variables by defining the dynamic causal estimands discussed by Beck and Katz (2011) in the potential outcomes framework for time series data (in the main text) and time series cross-sectional data (in the Supplemental Materials, Section C). Define Y as the theoretical value of the dependent variable. Index this variable by its location in time using $t \in \{1, \ldots, T\}$, such that Y_t is the theoretical value of the dependent variable in the contemporaneous time period and Y_{t+s} is the theoretical value of the dependent variable in a future time period for all $s \in \mathbb{N}$. Allow the index to be defined by two numbers separated by a colon such that the entirety of the time series of the dependent variable can be written as $Y_{1:T} = (Y_1, ..., Y_T)$. Similarly, a subset that series can be defined as $Y_{a:b} = (Y_a, Y_{a+1}, ..., Y_{b-1}, Y_b)$.

Define X as the theoretical value of an independent variable of interest that is causally prior to Y, and x as a potential realization of the independent variable. Given these definitions, let $Y_t(x_{1:T})$ be the potential outcome of the dependent variable that would be observed if a particular realization of the time series for the independent variable occurs. Similarly, define $\mathbf{W}$ as the theoretical values of a set of confounding variables $\boldsymbol{Z}$ that are causally prior to both X and Y, and $\mathbf{w}$ be potential realizations of those series. Allow the independent variable and confounding variables to be indexed the same way as the dependent variable. Finally, define $1_{0:s}$ as a series of length $s + 1$ in which all values equal 1, where s is the number of time periods since the start of a counterfactual treatment history.

With our notation defined, we can now define our theoretical estimands which appear in

Table 1: Dynamic Causal Estimands for Time Series Analysis

Causal Estimand	Time Series Nomenclature	Theoretical Formula
Instantaneous Treatment Effect	Short-Run Effect	$E\left[\begin{array}{l} Y_t(x_{1:t-1}, x_t+1, x_{t+1:T}) - \\ Y_t(x_{1:T}) \end{array}\right]$
Pulse Treatment Effect	Impulse Response Function	$E\left[\begin{array}{l} Y_{t+s}(x_{1:t-1}, x_t+1, x_{t+1:T}) - \\ Y_{t+s}(x_{1:T}) \end{array}\right]$
Step Treatment Effect	Unit Response Function	$E\left[\begin{array}{l} Y_{t+s}(x_{1:t-1}, x_{t:T}+1_{0:T-t}) - \\ Y_{t+s}(x_{1:T}) \end{array}\right]$

Table 1.[2] The Instantaneous Treatment Effect (ITE) is the difference in potential outcomes for the contemporaneous time period when the independent variable experiences a one-time, one-unit increase in the contemporaneous time period. In the time series literature, this is often called the short-run effect. Though important to define, this is a static causal estimand, as it does not study the effect of a counterfactual change to the contemporaneous independent variable on future potential values of the dependent variable. In addition to the static causal estimand of the ITE, we define two dynamic causal estimands. We motivate these with two types of economic activity that are thought to influence U.S. presidential approval. First, consider a short-run run event that occurs in one time period and then immediately recedes, like a shock in gasoline prices: a supply shock causes a short-term spike in prices that quickly returns to normal as supply returns to normal. We use this idea of a shock to define the Pulse Treatment Effect (PTE), the average effect a one-time, one-unit increase in the independent variable at time t has on the difference in potential outcomes at time period $t+s$. In the context of dynamic regression models, this is known as the Impulse Response Function.

Second, consider an event that, once it starts, permanently becomes part of a political system, like the level of unemployment: a technological change can cause unemployment to be

[2]We are heavily influenced by the Average Causal Effect as defined by Angrist and Pischke (2009), which was created for cross-sectional data, and the Lagged Effect of Treatment and Step Response of Treatment by Blackwell and Glynn (2018), which was created for time series cross-sectional data.

permanently higher due to the elimination of a previously necessary job. We define this as the Step Treatment Effect (STE), which is the average effect a permanent one-unit increase in the independent variable beginning at time t has on the difference in potential outcomes at time $t+s$. In the literature on dynamic regression models, this quantity goes by multiple names: either the Step Response Function or the Cumulative Impulse Response Function (Beck and Katz, 2011; Linn, Lebo and Webb, 2025). In Figure 1, we provide a visual representation of the counterfactual treatment histories that are added to the independent variable when defining the Pulse Treatment Effect and Step Treatment Effect.[3] The estimates, as well as their standard errors, were calculated using the `tseffects` package, an `R` package that can calculate the dynamic causal estimands presented in the paper for dynamic linear models.

3 Identifying Dynamic Causal Estimands

As mentioned, the dynamic causal estimands in Table 1 should be familiar to users of time series and time series cross-sectional data, regardless of their preferred research design. But there are multiple strategies scholars could use to identify these theoretical estimands. We focus on an identification strategy nested within the selection-on-observables research design for two reasons. First, some of the identification assumptions of selection-on-observables designs will later be used to explain why unit roots are a problem for causal inference and how scholars can address that problem within their own research design. Second, our empirical application applies our insights using dynamic regression models, which is one of many examples of selection-on-observables research designs. In the main text, we limit ourselves to discussing the identification assumptions, which are displayed in Table 2. A proof detailing how our assumptions enable identification of empirical estimands consistent with dynamic regression models is provided in the Supplemental Materials, Section A.

[3]The DGP was an ADL(1,0) dynamic linear model, $y_t = 0.5y_{t-1} + 1x_t + \varepsilon_t$, $t = 500$, $x_t, \varepsilon_t \sim N(0,1)$.

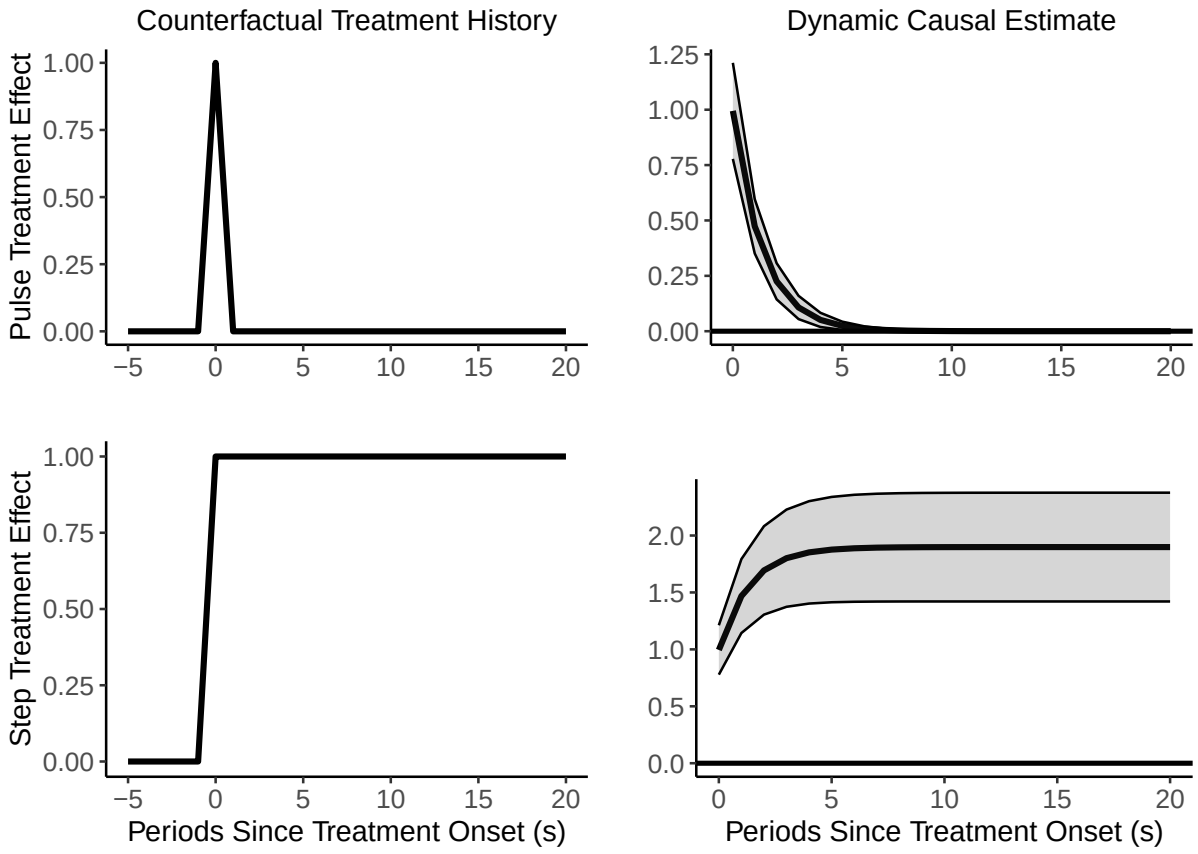

Figure 1: Counterfactual Treatment History and Dynamic Causal Estimates of the Pulse and Step Treatment Effects

Note: counterfactual treatment histories defining the Pulse Treatment Effect and Step Treatment Effect are depicted in the left column; estimates of the treatment effects for an artificial data-generating process are in the right column.

The first set of assumptions are alternatives to the Stable Unit Treatment Value Assumption that specifically refer to the time series properties of the data. The Consistency Assumption states that the theoretical value of the dependent variable equals the potential value of the dependent variable for a particular history of the independent variable when that history of the independent variable is observed. This component of the Stable Unit Treatment Value Assumption is practically universal to identification strategies in the potential outcomes framework and can be viewed as ruling out hidden variation in the independent variable that might be the result of measurement error, whether intentional or unintentional. Xu (2023) gives the example of a study on the effect of voter ID laws on voter participation in the U.S. In order to use a preferred research design, a scholar may be tempted to dichotomize states into two categories: those that adopt voter ID laws and those that do not. But there is substantial variation in the types of voter ID laws adopted in the U.S. If that variation matters in terms of potential outcomes, then the act of dichotomizing the independent variable violates the consistency assumption. This possibility further reinforces the need for dynamic causal estimands for nonbinary independent variables.

The Non-Anticipation Assumption states that potential outcomes of the dependent variable are not influenced by future values of the independent variable, $x_{t+s+1:T}$. This assumption is common in many approaches to causal inference with time series and time series cross-sectional data (Blackwell and Glynn, 2018; Bojinov and Shephard, 2019; Menchetti, Cipollini and Mealli, 2023; Bojinov, Simchi-Levi and Zhao, 2023). The Diminishing Carryover Effects Assumption states that the carryover effect of a past value of the independent variable on a future value of the potential outcome of the dependent variable eventually becomes zero as the amount of time between the two increases. This assumption is weaker than the Finite Carryover Effects Assumption in other approaches, which does not invoke limit behavior and states that the effect of past values of the independent variable drops sharply to zero after a finite period of time (Bojinov, Simchi-Levi and Zhao, 2023; Liu, Wang and Xu,

Table 2: Identification Assumptions for the Empirical Estimands in Table 1

Assumption	Formula	Plain Meaning
Consistency	$Y_{t+s} = Y_{t+s}(x_{a:b})$ if $X_{a:b} = x_{a:b}$	There is no hidden variation in x
Non-Anticipation	$Y_{t+s}(x_{1:T}) = Y_{t+s}(x_{1:t+s})$	Observations cannot anticipate changes to future values of x
Diminishing Carryover Effects	$\lim_{q+s\to\infty} Y_{t+s}(x_{1:t-q-1}, x_{t-q:T}) = Y_{t+s}(x_{t-q:T})$	The long-run effect of a shock to x on Y is ultimately 0
Observed Confounders	$X_{t:t+s} \perp\!\!\!\perp Y_{t+s}(x_{t-q:t+s}) \mid \boldsymbol{Z}$	Confounding variables are observed and controlled for
Conditional Positivity	$Pr(x_{t:t+s}\mid \mathbf{z}) > 0$	A given treatment history is possible for each time period

2024).[4] As we describe in the Supplemental Materials, Section A, the Diminishing Carryover Effects Assumption is necessary to allow for general forms of autoregression among potential outcomes; the Finite Carryover Effects Assumption does not allow for such autoregression.

The choice of an identification strategy that allows for autoregression is not motivated by a commitment to the dynamic regression model as an estimator. Rather, it is a theory driven choice informed by the prevalence of path dependency in political science, in which political outcomes in the present strongly resemble political outcomes in the past (Pierson, 2000). Many political phenomena are remarkably consistent over time, whether that is the partisan identification of individuals or the public policies of governments. This consistency is the result of increasing relative returns for continuing a particular political behavior, as such behavior creates negative externalities that discourage change and positive externalities that encourage persistence. Such a process causes outcome dependency, in which political decisions made in the present are a function of previous decisions (Page, 2006). Outcome dependence implies that a statistical model of that phenomena will necessarily include a

[4]The formula for the Finite Carryover Effects Assumption is $Y_{t+s}(x_{1:t-q-1}, x_{t-q:T}) = Y_{t+s}(x_{t-q:T})$.

form of autoregression, or the dependent variable will be a function of its own lag (Freeman and Jackson, 2012). Identification strategies that rely on Finite Carryover Effects are not credible for many political phenomena, and scholars invoking such an assumption should investigate whether the Diminishing Carryover Effects Assumption can be integrated into their preferred research designs.

The final two assumptions are generic to selection-on-observables research designs with minor adaptations to the dynamic context. The Observed Confounders Assumption states that the potential outcome of the dependent variable, when conditioned by a set of control variables $\boldsymbol{Z}$, is independent of the history of the independent variable. The set of control variables here is important, and we define them to include a set of confounding variables $\boldsymbol{W}_t$, their lags $\boldsymbol{W}_{t-q:t-1}$, lags of the independent variable $X_{t-q:t-1}$, and lags of the dependent variable $Y_{t-p:t-1}$.[5] Finally, the Conditional Positivity Assumption states that the probability of a given history of the independent variable conditional on the controls must be positive. This assumption encourages scholars to refrain from making inferences outside the variation of their data.

4 The Lingering Problem of Integration

With our dynamic causal estimands theoretically defined and empirically identified, we turn next to the issue of integration. For dynamic linear models to be consistently estimated, the dependent variable must be covariance stationary: the mean and variance of the dependent variable is not conditioned on time, and the covariance between the series and its lags is also not conditioned on time. While there are many potential violations of stationarity—such as deterministic trends and structural breaks—the violation of stationarity that has occupied

[5]Stated formally, $\boldsymbol{Z} = \{\boldsymbol{W}_{t-q:t}, X_{t-q:t-1}, Y_{t-p:t-1}\}$. For the sake of clarity, we assume that the number of required lags for the independent variable is the same as the number of required lags for the confounding variables. This need not be the case in actual applications; the number of lags for the confounding variables may be different than the number of lags for the independent variables.

the most scholarly attention in political science over the past decade is the threat of unit roots in integrated data. A time series has a unit root if its characteristic equation has a root on the unit circle. The most common example of a unit root is a random walk, or a first-order autoregressive variable in which the autoregressive parameter is equal to 1. A random walk written in potential outcomes notation is shown in Equation 1.[6]

$$Y_t(x_{1:T}) = Y_{t-1}(x_{1:T}) + \mu_t(x_{1:T}) + \varepsilon_t \tag{1}$$

The causal implication of a unit root is that the carryover effect of the independent variable on the dependent variable does not decline to zero over time. Rather, the independent variable is permanently integrated into the time series such that it always has a non-zero carryover effect on the dependent variable. As such, unit roots necessarily violate the Diminishing Carryover Effects Assumption stated earlier as well as its stronger counterpart, the Finite Carryover Effects assumption. Unit roots can be removed from a time series by first-differencing the series, or taking the difference between a time series and its first lag ($Y_t(x_{1:T}) - Y_{t-1}(x_{1:T}) = \Delta Y_t(x_{1:T})$).[7] Because of this, a time series is said to be integrated to order d ($Y_t \sim I(d)$) if the series must be differenced d times ($\Delta^d y_t$) before all unit roots are removed and the series is trend stationary.[8]

The consequences of using integrated data in dynamic linear models are staggering. For these models, integrated data causes the Central Limit Theorem to no longer apply to ordinary least squares, resulting in inconsistent estimates of coefficients. In addition, hypothesis tests will not have a limiting distribution, causing spurious significant associations to be de-

[6]While not the focus of this article, a stochastic random walk is a first-order autoregressive parameter where the parameter is allowed to change over time but has a mean of one. Stochastic unit roots cause similar problems in dynamic linear models and can be resolved using similar solutions (Yoon, 2006; Ginker and Lieberman, 2021).

[7]This assumes nonseasonal unit roots, an assumption we make throughout the article for clarity. It would be trivial to extend our results to seasonal unit roots.

[8]We assume $d \in \mathbb{N}$ and do not consider the possibility of fractional integration in this manuscript.

tected between unrelated series at levels higher than nominal (Granger and Newbold, 1974; Linn, Lebo and Webb, 2025). To avoid this problem, the dependent variable is stationary and that the equation is balanced: every regressor must either itself be $I(0)$ or, when taken as a linear combination of other regressors, must produce a variable that is $I(0)$ (Pickup and Kellstedt, 2023).

Whenever variables in a dynamic regression model are integrated but not cointegrated, the most commonly used solution for removing the unit root is to difference the $I(d)$ variable d times and include the differenced variable in dynamic linear model. By doing so, equation balance can be achieved and dynamic linear models can still be estimated. But this creates consequences for causal inference. When the dependent variable or independent variable must be differenced, the model recovers estimates of dynamic causal estimands that feature the variable in differences, rather than the variable in levels. These reflect inferences regarding relationships solely in short-term changes; while useful to some extent, analysts prefer to have estimands of the variables in levels. This is why the GECM parametrization of the dynamic linear model is so widely celebrated: it allows for causal inferences in levels even if the dependent and independent variables are integrated, so long as they are cointegrated. But if no such cointegration exists, scholars are limited to inferences about short-run relationships.

In contrast, the discussion of stationarity for research designs using time series data beyond the dynamic linear model has been much more limited. This is unfortunate, as stationarity is important and consequential for virtually all research designs using such data. Multiple research designs are based on dynamic regression models, such as vector dynamic regression models and structural nested mean models. They explicitly require stationarity in the dependent and independent variables to make valid inferences. Similarly, intervention analysis explicitly assumes difference stationarity of the dependent variable (Box and Tiao, 1975; Menchetti, Cipollini and Mealli, 2023). Marginal structural models with inverse

probability of treatment weighting will also not be identified to the extent they rely on a version of the Diminishing Carryover Effects or Finite Carryover Effects Assumptions. Indeed, marginal structural models require the estimation of the probability density function of the independent variable conditional on the confounders, which implicitly requires a model of the independent variable's conditional mean. This simply shifts the burden of integrated variables to the independent variable and confounding variables, which could potentially include the lagged dependent variable. Differencing of integrated variables prior to effect estimation may yet be required for marginal structural models.[9]

Stationarity is similarly important for time series cross-sectional research designs. The synthetic control method and related research designs are consistent when the dependent variable of the treated unit is cointegrated with the dependent variable of the control units. But when the treated unit and control units are generated from independent random walks - or are integrated but not cointegrated - these research designs produce inconsistent estimates and result in spurious inferences; a suggested solution in this case is to difference the dependent variable until it is stationary (Masini and Medeiros, 2022). In difference-in-difference estimators, stationarity of potential outcomes for the untreated is sometimes necessary for parallel trends to hold when treatment is a dynamic choice made by rational agents (Marx, Tamer and Tang, 2024). Indeed, mean stationarity of potential outcomes for the untreated is an alternative identification assumption in a variety of circumstances, which allows for a form of doubly-robust identification if either parallel trends or mean stationarity holds. Given this possibility, differencing an integrated dependent variable before using differences-in-differences seems like an attractive research design.

The consequences of unit roots even reaches as far as dynamic panel regression models. Unlike its time series counterpart, dynamic panel regression models can consistently estimate

[9]While implied by the statistical foundations of the technique, there is no Monte Carlo evidence demonstrating this claim. We leave its investigation to future research.

the relationship between a unit root independent variable and dependent variable so long as regression coefficients are assumed to be homogeneous across units (Phillips and Moon, 1999). But this is a heavily criticized assumption of regression models, with heterogeneous treatment effects across individuals being a more plausible alternative (Aronow and Samii, 2016). In the face of coefficient heterogeneity, dynamic panel regression models are inconsistent (Pesaran and Smith, 1995; Pesaran, 2015). The most robust solution to estimate dynamic causal estimands in this scenario would be to disaggregate the panel data into N separate time series, but this would just reinforce the need to difference variables with unit roots.

5 Solving the Problem: the Generalized Dynamic Treatment Effect

In short: integration is a problem, and differencing is the solution. But differencing has an interpretational cost: we must make inferences about our variables in differences when we would like to make inferences about our variables in levels. We propose that this cost can be eliminated by generalizing the dynamic causal estimands previously been discussed in Table 1. Equation 2 introduces the Generalized Dynamic Treatment Effect, $\text{GDTE}_{s,h,d.y,d.x}$. This is the difference in potential outcomes for a differenced dependent variable due to a counterfactual treatment history applied to a differenced independent variable. An identification analysis using the same assumptions as before is presented in the Supplemental Materials, Section B. As before, $s \in \mathbb{N}$ is the number of time periods until treatment. $d.x \in \mathbb{N}$ is the amount of times an independent variable is differenced before a counterfactual treatment is added. Similarly, $d.y \in \mathbb{N}$ is the amount of times the potential outcome is differenced before determining the effect of the treatment history on the dependent variable is calculated.

$$\text{GDTE}_{s,h,d.y,d.x} \equiv E[\Delta^{d.y} Y_{t+s}(\Delta^{d.x} x_{1:t-1}, \Delta^{d.x} x_{t:T} + c_{0:T-t,h}) - \Delta^{d.y} Y_{t+s}(\Delta^{d.x} x_{1:T})] \qquad (2)$$

Table 3: Possible Counterfactual Series $c_{0:s,h}$ Added to the Generalized Dynamic Treatment Effect, by h

Order Number	Numeric Sequence	Mathematical Nomenclature	Substantive Interpretation of Treatment's Change to X
$h \geq 0$	$\binom{h+s}{s}$	Diagonal h of Pascal's triangle	Sum of first n deterministic trend coefficients become one-unit larger
$\vdots$	$\vdots$	$\vdots$	$\vdots$
2	$(1, 3, 6, 10, \dots)$	Triangular numbers	Linear and quadratic trend coefficients of X become $\frac{1}{2}$-unit larger each
1	$(1, 2, 3, 4, \dots)$	Natural numbers	Linear trend coefficient of X becomes one-unit larger
0	$(1, 1, 1, 1, \dots)$	Step function	Permanent one-unit increase
-1	$(1, 0, 0, 0, \dots)$	Pulse function	One-time, one-unit increase
-2	$(1, -1, 0, 0, \dots)$	Transient intervention function	Treatment lasts two periods and the cumulative treatment is zero
$\vdots$	$\vdots$	$\vdots$	$\vdots$
$h < 0$	$(-1)^s \binom{\lvert h \rvert - 1}{s}$	Oscillating row $\lvert h \rvert - 1$ of Pascal's triangle	Treatment lasts $\lvert h \rvert$ periods and the cumulative treatment is zero

The only other parameter, $h \in \mathbb{I}$, is the treatment history parameter; it determines the counterfactual series $c_{0:s,h}$ that is added to the independent variable for inference. To introduce the Generalized Dynamic Treatment Effect more descriptively, Table 3 contains a summary of the possible counterfactual series for the GDTE. Each of the counterfactual series $c_{0:s,h}$ begins at $c_{0,h} = 1$. Treatment never begins prior to this point; thus $c_{-\infty:-1,h} = 0_{1:\infty}$. Each subsequent value of $c_{1:s,h}$ is generated by applying the binomial coefficient $\binom{n}{k}$ using the parameters h and s. Because they are generated using the binomial coefficient, the counterfactual series are recursively related as shown in Equation 3.[10]

$$c_{s,h-m} = \sum_{j=0}^{s} (-1)^j \binom{m}{j} c_{s-j,h} \quad \forall s, m \in \mathbb{N} \tag{3}$$

[10] A proof of this equation is given in the Supplemental Materials, Section D.

The GDTE houses the two dynamic causal estimands from Table 1. When $h = -1$, the GDTE equals the Pulse Treatment Effect. Similarly, the GDTE equals the Step Treatment Effect when $h = 0$. Beyond these two cases, negative numbers indicate a treatment regimen that will terminate in $|h|$ periods and cancel out in the long-run.[11] Positive numbers, in contrast, indicate treatments that will continuously grow larger over time. Finally, we note that when $s = 0$, the GDTE reduces to the Instantaneous Treatment Effect for all values of h.

The GDTE is useful because it accounts for many realistic counterfactual treatment histories for independent variables. Table 3 shows it allows for the calculation of the PTE and STE from Table 1. But it also allows further treatment histories: the GDTE at $h = 1$, which could colloquially be called the linear trend treatment effect, would describe what would happen to the dependent variable if the independent variable trended one unit faster each time period. Some independent variables, like inflation, are known to have deterministic trends. The linear trend treatment effect would describe what would happen to the dependent variable if inflation grew by an additional percentage point each time period.

In some instances, of course, multiple dynamic causal estimands will be theoretically interesting. Consider the effect that salaries have on the retention of public sector employees (Grissom, Viano and Selin, 2016). A step effect to salary could be interpreted as the effect a pay increase would have on employee retention, while a pulse effect could be interpreted as the effect of a one-time bonus. Because both quantities have a theoretical interpretation, scholars might choose to estimate both as part of a single research design. The GDTE invites users to choose the most theoretically compelling causal estimand(s) appropriate to their independent variable.

The GDTE is a useful, unifying framework for considering common treatment histories for dynamic analysis. But its value is far greater: scholars can use the GDTE to make

[11] For $h < -1$, $\lim_{s\to\infty} \sum_{i=0}^{s} c_{i,h} = 0$.

inferences about variables in levels even if their research design requires such variables to be differenced. This is because a differenced variable can be expressed using binomial coefficients, as shown in Equation 4; this is similar to the recursive expression of the counterfactual treatments in the GDTE, as shown in Equation 3. We begin by discussing how a differenced independent variable can be used to make inferences about that independent variable in levels, before transitioning to the same task for a dependent variable. This is primarily done by demonstrating identities, either for the Generalized Dynamic Treatment Effect or for dynamic causal estimands more generally.

$$\Delta^{d.y} y_t = \sum_{i=0}^{d.y} (-1)^i \binom{d.y}{i} y_{t-i} \tag{4}$$

When the independent variable is integrated of order $d.x$ but is not cointegrated with any other variables in the model, the independent variable must be differenced $d.x$ times in order to become stationary. If the differenced independent variable is used to estimate the GDTE, the estimand will summarize the causal effect of a counterfactual treatment of the differenced independent variable on the dependent variable. But recall the the $d.x$ parameter defined for $\text{GDTE}_{s,h,d.y,d.x}$ in the previous section summarizes the differencing that happens before a treatment is applied; thus, $(\Delta^{d.x} x_{t+s}) + c_{t+s,h}$. While such an estimand may be desirable in some contexts, many scholars would prefer an estimand featuring a counterfactual treatment to the independent variable in levels, even if the independent variable itself has to be differenced $d.x$ times in order to produce valid estimates in the dynamic model. This implies the desired estimand is $\text{GDTE}_{s,h,d.y,0}$, with treatment $\Delta^{d.x}((\Delta^0 x_{t+s}) + c_{t+s,h}) = \Delta^{d.x}(x_{t+s} + c_{t+s,h})$. By using Equations 3 and 4, we see that an arbitrary counterfactual treatment history for a variable in levels is equivalent to another counterfactual treatment to that same variable

after differencing:

$$
\begin{aligned}
\text{GDTE}_{s,h,d.y,0} &\equiv E[\Delta^{d.y}Y_{t+s}(\Delta^{d.x}(x_{1:t-1}), \Delta^{d.x}(x_{t:T} + c_{0:T-t,h})) - \Delta^{d.y}Y_{t+s}(\Delta^{d.x}(x_{1:T}))] \\
&= E[\Delta^{d.y}Y_{t+s}(\sum_{i=0}^{d.x}(-1)^i \binom{d.x}{i}(x_{1:t-1}), \sum_{i=0}^{d.x}(-1)^i \binom{d.x}{i}(x_{t:T} + c_{0:T-t,h}))- \\
&\quad \Delta^{d.y}Y_{t+s}(\sum_{i=0}^{d.x}(-1)^i \binom{d.x}{i}(x_{1:T}))] \\
&= E[\Delta^{d.y}Y_{t+s}(\sum_{i=0}^{d.x}(-1)^i \binom{d.x}{i}x_{1-i:t-1-i}, \sum_{i=0}^{d.x}(-1)^i \binom{d.x}{i}x_{t-i:T-i}+ \\
&\quad \sum_{i=0}^{d.x}(-1)^i \binom{d.x}{i}c_{0-i:T-t-i,h})) - \Delta^{d.y}Y_{t+s}(\sum_{i=0}^{d.x}(-1)^i \binom{d.x}{i}x_{1-i:T-i})] \\
&= E[\Delta^{d.y}Y_{t+s}(\Delta^{d.x}x_{1:t-1}, \Delta^{d.x}x_{t:T} + c_{0:T-t,h-d.x}) - \Delta^{d.y}Y_{t+s}(\Delta^{d.x}x_{1:T})] \\
&= \text{GDTE}_{s,h-d.x,d.y,d.x}
\end{aligned}
$$

$$
\text{GDTE}_{s,h,d.y,d.x} = \text{GDTE}_{s,h+d.x,d.y,0} \tag{5}
$$

The forgoing proof demonstrates that the GDTE for treatment h applied to an independent variable differenced $d.x$ times can be equivalently interpreted as the GDTE for treatment $h + d.x$ applied to that same independent variable in levels. Importantly, the validity of Equation 5 does not depend on the use of any particular estimator nor rely on any particular identification assumptions. It is simply true given the recursive relationship between the possible treatment histories under the GDTE.[12] The result implies a strategy that all scholars can use to recover dynamic causal estimands in levels even with an integrated independent variable. Using the scholar's preferred research design, simply difference the

[12]We expand on this recursive relationship in the Supplemental Materials, Section D.

independent variable as normal and, rather than estimate $\text{GDTE}_{s,h,d.y,d.x}$ and making inferences in differences, estimate $\text{GDTE}_{s,h-d.x,d.y,d.x}$ and interpret the quantity as $\text{GDTE}_{s,h,d.y,0}$ so that inferences can be made in levels.

When the dependent variable is integrated of order $d.y$ but is not cointegrated with any other variables in the model, the dependent variable must be differenced $d.y$ times in order to become stationary. If the differenced dependent variable is used to estimate Generalized Dynamic Treatment Effect, the estimand will summarize the causal effect of a counterfactual treatment of the independent variable on the differenced dependent variable. While such an estimand may be desirable in some contexts, many scholars would prefer an estimand featuring a treatment effect on the dependent variable in levels, even if the dependent variable itself has to be differenced $d.y$ times in order to produce valid estimates of this quantity. It is possible to recover an empirical estimand of a particular counterfactual treatment history on the dependent variable in levels by using estimands of that causal effect on the dependent variable in differences. This can be demonstrated using the linearity of conditional expectations. We demonstrate this with the Generalized Dynamic Treatment Effect, noting that this formula would apply to any estimand using expectations. We start

with the case of a first differenced dependent variable:

$$
\begin{aligned}
\text{GDTE}_{s,h,1,d.x} &\equiv E[\Delta Y_{t+s}(\Delta^{d.x} x_{1:t-1}, \Delta^{d.x} x_{t:T} + c_{0:T-T,h}) - \Delta Y_{t+s}(\Delta^{d.x} x_{1:T})] \\
&= E[(Y_{t+s}(\Delta^{d.x} x_{1:t-1}, \Delta^{d.x} x_{t:T} + c_{0:T-T,h}) - Y_{t+s-1}(\Delta^{d.x} x_{1:t-1}, \Delta^{d.x} x_{t:T} + c_{0:T-T,h}))- \\
&\quad (\Delta Y_{t+s}(\Delta^{d.x} x_{1:T}) - \Delta Y_{t+s-1}(\Delta^{d.x} x_{1:T}))] \\
&= E[(Y_{t+s}(\Delta^{d.x} x_{1:t-1}, \Delta^{d.x} x_{t:T} + c_{0:T-T,h}) - Y_{t+s}(\Delta^{d.x} x_{1:T}))- \\
&\quad (Y_{t+s-1}(\Delta^{d.x} x_{1:t-1}, \Delta^{d.x} x_{t:T} + c_{0:T-T,h}) - Y_{t+s-1}(\Delta^{d.x} x_{1:T}))] \\
&= E[Y_{t+s}(\Delta^{d.x} x_{1:t-1}, \Delta^{d.x} x_{t:T} + c_{0:T-T,h}) - Y_{t+s}(\Delta^{d.x} x_{1:T})]- \\
&\quad E[Y_{t+s-1}(\Delta^{d.x} x_{1:t-1}, \Delta^{d.x} x_{t:T} + c_{0:T-T,h}) - Y_{t+s-1}(\Delta^{d.x} x_{1:T})] \\
&= \text{GDTE}_{s,h,0,d.x} - \text{GDTE}_{s-1,h,0,d.x} \\
\text{GDTE}_{s,h,0,d.x} &= \text{GDTE}_{s,h,1,d.x} + \text{GDTE}_{s-1,h,0,d.x}
\end{aligned}
$$

Initially, it would appear that this equation is impractical; if $\text{GDTE}_{s,h,0,d.x}$ is unknown, then $\text{GDTE}_{s-1,h,0,d.x}$ must also be unknown. But one of the most prominent assumptions used when identifying dynamic causal estimands is the non-anticipation of treatments, as displayed in Table 2. Using this identification assumption alone, $\text{GDTE}_{s-1,h,0,d.x} = 0$ whenever $s \leq 0$. As such, we can calculate $\text{GDTE}_{0,h,1,d.x} = \text{GDTE}_{0,h,0,d.x}$. And using this value as a baseline, we can calculate the $\text{GDTE}_{s,h,0,d.x}$ for any natural number s. This result is a generalization of a result previously found by Menchetti, Cipollini and Mealli (2023) for the Step Treatment Effect. This proof concept can be extended to higher-order differencing, resulting in the more general Equations 6 and 7. Importantly, the formulae and their application does not depend on any particular research design. It only relies on the Non-Anticipation Assumption. And while we limit ourselves to using the Non-Anticipation Assumption here, we note that it would be possible to use this same strategy to calculate the GDTE in levels with a weaker assumption that allows for finite anticipation effects.

$$\text{GDTE}_{s,h,d.y,d.x} = \sum_{i=0}^{s} (-1)^i \binom{d.y}{i} \text{GDTE}_{s-i,h,0,d.x} \tag{6}$$

$$\text{GDTE}_{s,h,0,d.x} = \sum_{i=0}^{s} \binom{d.y + i - 1}{i} \text{GDTE}_{s-i,h,d.y,d.x} \tag{7}$$

6 Incorporating the Workhorse: Empirical Estimands and Dynamic Linear Models

While a myriad of research designs are available to scholars who want inferences from dynamic data, by far and away the most common modeling approach is the dynamic linear model: either the ADL or GECM. The popularity of these models largely stems from the fact that they are single-equation regression models: easy to estimate and well understood. The ADL(p, q) model with r moving averages, as shown in Equation 8, assumes the conditional mean of the dependent variable in levels is a linear function of lags of the dependent variable Y, independent variable X, confounding variables W, and error term ε, all of which are in levels (Vande Kamp and Jordan, 2025). The GECM$(p-1, q-1)$, as shown in Equation 9, is a reparameterization of the ADL(p, q) model using lags of the covariates in both levels and in first-differences.

$$Y_t = \alpha_0 + \sum_{i=1}^{p} \alpha_i Y_{t-i} + \sum_{j=0}^{q} \beta_j X_{t-j} + \sum_{j=0}^{q} \gamma_j W_{t-j} + \sum_{k=0}^{r} \theta_k \varepsilon_{t-k} \tag{8}$$

$$\Delta Y_t = \alpha_0 + \sum_{i=1}^{p-1} \alpha_i \Delta Y_{t-i} + \theta_0 Y_{t-1} + \sum_{j=0}^{q-1} \beta_j \Delta X_{t-j} + \theta_1 X_{t-1} + \sum_{j=0}^{q} \gamma_j W_{t-j} + \sum_{k=0}^{r} \theta_k \varepsilon_{t-k} \tag{9}$$

Because the usage of dynamic regression models in political science considerably predates

Table 4: Formulae for Deriving Causal Estimands from Dynamic Linear Models

Causal Estimand	Time Series Nomenclature	ADL Formula	GECM Formula
Instantaneous Treatment Effect	Short-Run Effect	$ITE = \beta_0$	$ITE = \beta_0$
Pulse Treatment Effect	Impulse Response Function	$PTE_s = \beta_s + \sum_{i=1}^{s} \alpha_i PTE_{s-i}$	$PTE_s = (\beta_s - \beta_{s-1}) + (\alpha_1 + 1 + \theta_0)PTE_{s-1} + \sum_{i=2}^{s}(\alpha_s - \alpha_{s-1})PTE_{s-i}$
Step Treatment Effect	Cumulative Impulse Response Function	$STE_s = \sum_{k=0}^{s} PTE_k = PTE_s + STE_{s-1}$	$STE_s = \sum_{k=0}^{s} PTE_k = PTE_s + STE_{s-1}$

Note: The formulae in columns 3 and 4 assume a dynamic linear model in the form of equations 8 and 9.

the attention to causal inference in the discipline, the formulae for calculating dynamic causal estimands from dynamic regression models has been available for decades (Box and Jenkins, 1976). We present the formulae for deriving each of these empirical estimands from dynamic linear models in Table 4.[13] The formulae are nonlinear combinations of regression coefficients; the exception is the Instantaneous Treatment Effect, which is simply β_0 for both the ADL(p, q) model and the GECM$(p-1, q-1)$. The Step Treatment Effect is sometimes called the Cumulative Impulse Response Function in the time series literature. This naming convention highlights one of the unique properties of dynamic linear models: the Step Treatment Effect is equal to the cumulative sum of the Pulse Treatment Effect. This property is imposed by the structure of the dynamic linear models in Equations 8 and 9. Because estimates of dynamic causal estimands from dynamic linear models are derived using nonlinear functions of coefficients, the standard error of an estimate can be approximated using the delta method or bootstrapping.

The causal identification assumptions in Table 2 go by different names in the time series literature. Weak exogeneity is a necessary assumption for inference using dynamic regres-

[13]The GECM formula for the Pulse Treatment Effect is valid for $h \geq 2$. For the case $h = 1$, the GECM formula is actually $PTE_1 = (\beta_1 - \beta_0 + \theta_1) + (\alpha_1 + 1 + \theta_0)PTE_0 = \beta_1 + \theta_1 + \beta_0(\alpha_1 + \theta_0)$.

sion models (Engle, Hendry and Richard, 1983). There are a number of examples that can cause a violation of this assumption: omitted variable bias, rational expectations, measurement error, and simultaneity (Linn, Lebo and Webb, 2025). These violations implies the assumptions of Observed Confounders, Non-Anticipation, and Consistency. Similarly, the requirement that independent and dependent variables are stationary implies the assumption of Diminishing Carryover Effects. Unfortunately, there does not seem to have a counterpart for the assumption of Conditional Positivity in the time series literature. This is certainly problematic, though it is unclear how a scholar would go about analyzing the conditional positivity assumption for a particular research design. More work is needed to demonstrate how scholars using dynamic regression models can test the Conditional Positivity Assumption.

Finally, while all selection-on-observables research designs struggle with observing all possible confounders $\boldsymbol{W}$, dynamic linear models are particularly well suited to include all necessary lags of X, Y, and $\boldsymbol{W}$. The general-to-specific methodology developed by Hendry (1995) for dynamic regression models employs rigorous post-estimation testing to ensure that a model includes all necessary lags for the Observed Confounders Assumption. Additionally, ADL(p, q) models can consistently estimate the PTE_s for all time points $s \in \{1, \ldots, q\}$, even if p is misspecified (Baek and Lee, 2022). Said differently, ADL models are robust to misspecification of the number of lags of Y so long as enough lags of X are included. These robustness properties are certainly a boon compared to alternative approaches that have to rely on sensitivity analysis.

Beyond the identification assumptions necessary for causal inference, the use of dynamic regression models for dynamic causal inference requires two additional assumptions. First, the relationship between X and Y must be capable of being expressed with linear parameters and must be uniform for all observations. Some have criticized this linearity assumption as too restrictive when there is effect heterogeneity (Aronow and Samii, 2016). Yet recent work demonstrates that multiplicative interactions can be meaningfully incorporated

into dynamic regression models (Warner, Vande Kamp and Jordan, Forthcoming). Indeed, the mean-centering of moderating variables allows scholars to estimate both Average Treatment Effects and Conditional Average Treatment Effects simultaneously (Chattopadhyay and Zubizarreta, 2023). While effect heterogeneity is not the focus of this paper, we discuss interesting avenues for future research in the conclusion.

Second, confounding variable(s) must be statistically independent from lagged values of the independent variable (Blackwell and Glynn, 2018). The scholar must assume that the independent variable does not Granger Cause the confounding variable(s). If this assumption is violated, a dynamic regression model cannot consistently estimate dynamic causal estimands, though it is still possible to estimate the static causal estimand known as the Instantaneous Treatment Effect. Fortunately, the structural nested mean model is an extension of the dynamic regression models that relaxes this assumption. Scholars must check to see if their independent variable Granger Causes any of their confounding variables and, if so, employ a more sophisticated alternative to make dynamic causal inferences.

With the assumptions for using dynamic regression models fully stated, we now turn to focusing on how to make causal inferences for integrated variables, specifically by using the ADL model with a differenced independent variable and dependent variable as shown in Equation 10. While our presentation focuses on the ADL model, we note that a GECM parameterization is possible and thus the results could extend to a GECM used for data with mismatched orders of integration.

$$\Delta^{d.y} Y_t = \alpha_0 + \sum_{i=1}^{p} \alpha_i \Delta^{d.y} Y_{t-i} + \sum_{j=0}^{q} \beta_j \Delta^{d.x} X_{t-j} + \sum_{j=0}^{q} \gamma_j W_{t-j} + \sum_{k=0}^{r} \theta_k \varepsilon_{t-k} \tag{10}$$

Recall that one of the properties of the dynamic linear model is that the Step Treatment Effect equals the cumulative sum of the Pulse Treatment Effect. This relationship results from the assumption in the dynamic linear model that the efficacy of the current treatment

is not dependent on the value of previous treatments. While this assumption can be used to quickly calculate the Step Treatment Effect, it is not limited to the Step Treatment Effect. Any dynamic causal estimand from Table 3 will be a function of the Pulse Treatment Effect when using the dynamic linear model. For example, we can exploit the recursive nature of the Generalized Dynamic Treatment Effect to provide the following formulae for their calculation from dynamic linear models, as shown in Equations 11 and 12. To emphasize the point: any GDTE will be a function of the Impulse Response Function.

$$\text{GDTE}_{s,h,0,0} = \sum_{j=0}^{s} \binom{j+h}{j} PTE_{s-j} = \sum_{j=0}^{s} \binom{j+h}{j} \text{GDTE}_{s-j,-1,0,0} \quad \forall s, h \in \mathbb{N} \tag{11}$$

$$\text{GDTE}_{s,h,0,0} = \sum_{j=0}^{s} (-1)^j \binom{|h|-1}{j} PTE_{s-j} = \sum_{j=0}^{s} (-1)^j \binom{|h|-1}{j} \text{GDTE}_{s-j,-1,0,0} \quad \forall s, -h \in \mathbb{N}_+ \tag{12}$$

Of great interest to practitioners of the ADL and GECM model, these formula can be further extended to enable inference about variables in levels using variables in differences, thus helping to "solve" the problem of causal inference with variables in differences. Equation 5 demonstrates that a GDTE applied to an independent variable in differences can be equivalently interpreted as a different GDTE applied to the same independent variable in levels. A similar result can be found for differenced dependent variables by assuming the dynamic linear model. As shown in the Supplemental Materials, Section G, and similar to Equation 12, one arrives at Equation 13:

$$\text{GDTE}_{s,h,d.y,d.x} = \text{GDTE}_{s,h-d.y,0,d.x} \tag{13}$$

Taken together, Equations 5 and 13 demonstrate that a dynamic linear model with differenced variables can be used to recover inferences for the Generalized Dynamic Treatment Effect in levels by adjusting the treatment history parameter h by the amount of differencing

applied to the dependent variable, $d.y$, and independent variable, $d.x$. Equations 14 and 15 generalize upon Equations 11 and 12 by including the parameters $d.y$ and $d.x$.[14] As before, the relevant equation depends on the combined values of h, $d.x$, and $d.y$.

$$\text{GDTE}_{s,h,0,0} = \sum_{j=0}^{s} \binom{j+h+d.y-d.x}{j} \text{GDTE}_{s-j,-1,d.y,d.x} \quad \forall s, h, d.x, d.y, (h+d.y-d.x) \in \mathbb{N} \tag{14}$$

$$\text{GDTE}_{s,h,0,0} = \sum_{j=0}^{s} (-1)^j \binom{|h|-1+d.x-d.y}{j} \text{GDTE}_{s-j,-1,d.y,d.x} \quad \forall d.x, d.y, s \in \mathbb{N}, |h|, (|h|+d.x-d.y) \in \mathbb{N}_+ \tag{15}$$

One interesting feature of these formulae is the role of $d.y$ and $d.x$. They both appear a single time in the numerator of the binomial coefficient, and nowhere else. Because they have different signs, the two parameters will cancel out when they are equal ($d.y = d.x$). From a practice perspective, this means that the estimates of the GDTE when both the independent variable and dependent variable are in differences will be identical to estimates of the GDTE when both variables are in levels, so long as the order of differencing in both variables is equal. This core result is reported in Equation 16. Importantly, it matches the well known equivalency between a fixed effects model estimated using dummy variables and a fixed effects model estimated using first differences of the dependent and independent variables (Pesaran, 2015).

$$\text{GDTE}_{s,h,d.y,d.x} = \text{GDTE}_{s,h,0,0} \quad \forall (d.y = d.x) \in \mathbb{N} \tag{16}$$

We demonstrate this result graphically by simulating data from an ADL(1,0) model in Equation 18 using the independent variable in Equation 17. The error terms in both equations are white noise. $d.x \in \{0, 1\}$ determines whether the independent variable is a unit root; if so, in enters the model for the independent variable in differences. $d.y \in \{0, 1\}$

[14] Many equations in the main text are presented in pairs for ease of exposition. A unified version of such equations is presented in the Supplemental Materials, Section E.

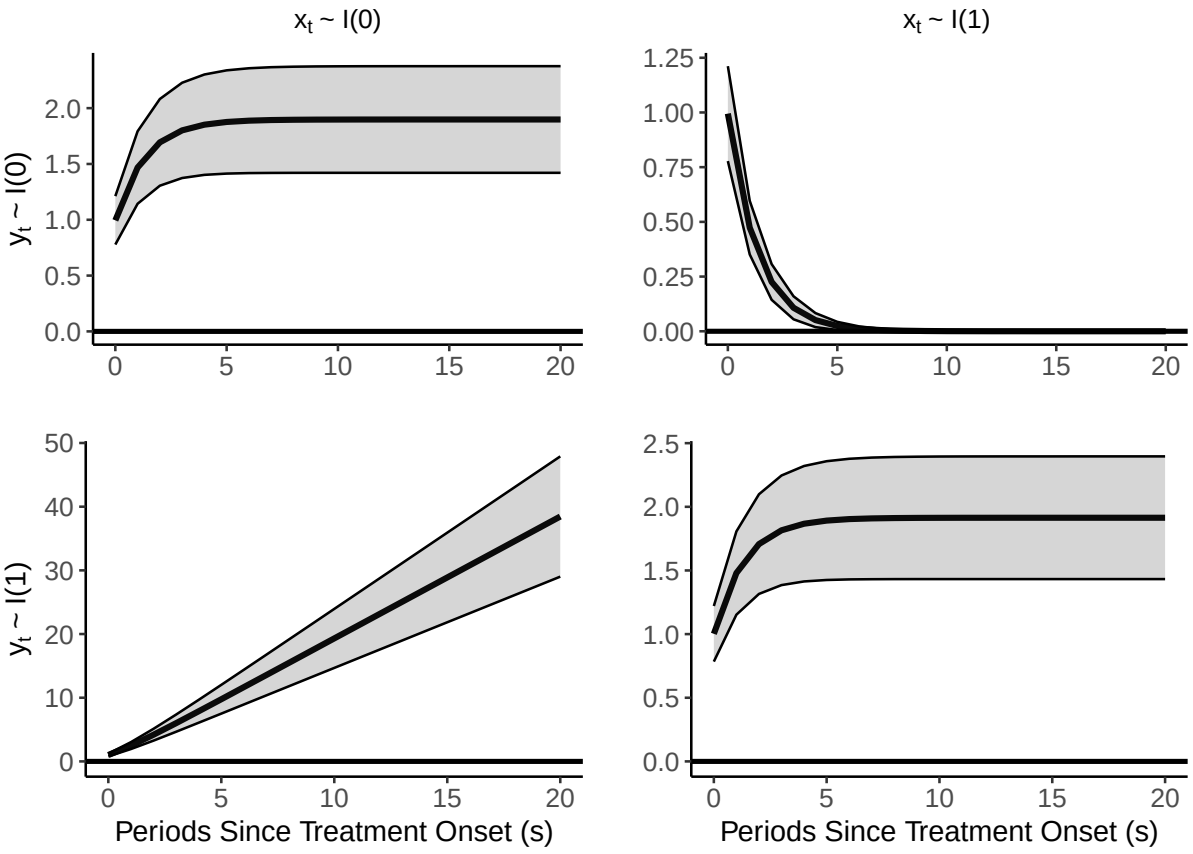

Figure 2: Estimates of the Step Treatment Effects from an ADL(1,0) model, by whether the Independent Variable or Dependent Variable have a Unit Root that Requires Differencing

determines if the dependent variable has a unit root; regardless of the answer, the dependent variable will also have an autoregressive component ($\alpha = 0.5$). To ensure our simulations are as consistent as possible, the error terms are first simulated, the independent variable is constructed, and finally the dependent variable is constructed. Afterwards, the variables are differenced as necessary and the correct ADL(1,0) model is fit.

$$x_t = (d.x)x_{t-1} + u_t \tag{17}$$

$$\Delta^{d.y} y_t = 1 + 0.5\Delta^{d.y} y_{t-1} + 1\Delta^{d.x} x_t + \varepsilon_t \tag{18}$$

Figure 2 presents estimates of the Step Treatment Effect for the models of simulated

data, created using the `tseffects` package in `R`. As seen in the diagonal from top-left to bottom-right, the estimates are identical whenever $d.x = d.y$, regardless of whether the variables have actually been differenced or not. For these data-generating processes, the Instantaneous Treatment Effect is approximately 1; this result is consistent with Equation 16. In the top-right corner of Figure 2, the independent variable is $I(1)$ while the dependent variable is $I(0)$. In this scenario, the Step Treatment Effect uses the formula for typically used for the Pulse Treatment Effect when all variables are stationary. In the bottom-left pane, featuring an $I(0)$ independent variable and an $I(1)$ dependent variable, the Step Treatment Effect uses the formula typically used for the linear trend treatment effect when all variables are stationary. The symmetry is obvious, despite the different methods of calculation.

7 Empirical Example

To demonstrate how a scholar would make causal inferences from nonstationary data, we focus on a study that uses an ADL(p, q) model with a differenced independent variable. Soroka (2006) seeks to explain media responsiveness to shifts in the economy by studying the effect of economic fluctuations in the United Kingdom. Soroka theorizes that people process information asymmetrically: new negative information will have a greater effect on perceptions and behavior than new positive information. While Soroka supports this theory with a variety of statistical tests, we focus on one pair of results in which he studies how monthly changes to the Conference Board's Leading Index affects the number of news stories that negatively portray the U.K. economy in *The Times* newspaper between 1986-2000. Because the Leading Index is a measure of the strength of the economy, he reverses its sign to indicate that increases to the index indicate that the economy is getting worse.

As reported in his Appendix, Soroka tests the variables for unit roots and finds that monthly economic indicators are $I(1)$ while media coverage is $I(0)$. And though unreported,

Soroka claims in a footnote that preliminary analysis indicated that media coverage is significantly autocorrelated with the first four lags of the series; such quarterly autocorrelation is common with monthly financial series. Because of this, he estimates a modified $ADL(4,2)$ model with a first-differenced independent variable and the dependent variable in levels. To test his theory, he estimates the model twice. The first model, reported in Equation 7, simply includes the independent variable $\Delta Econ_{t-2}$ as is. The second model, reported in Equation 7, breaks the independent variable into two: a variable $\Delta Econ(Worse)_{t-2}$ that equals $\Delta Econ_{t-2}$ when the economy is getting worse and zero otherwise, and a variable $\Delta Econ(Better)_{t-2}$ that equals $\Delta Econ_{t-2}$ when the economy is getting better and zero otherwise. Based on his theory, he hypothesizes that the effect estimates for a negative downturn in the economy will be greater in the latter model because it is not bogged down by the muted responses that occur when the economy improves. The results of these models are reported in his Table 2 (p. 378) and our Supplemental Materials, Section H.

$$Media_t = \alpha_1 + \sum_{i=1}^{4} Media_{t-i} + \Delta Econ_{t-2} + \varepsilon_t$$

$$Media_t = \alpha_1 + \sum_{i=1}^{4} Media_{t-i} + \Delta Econ(Better)_{t-2} + \Delta Econ(Worse)_{t-2} + \varepsilon_t$$

To communicate his results, Soroka approximates the Pulse Treatment Effect of a standard deviation worsening in the economy for both models and plots how it unfolds over time in his Figure 2 (p. 382). Such an effort puts him ahead of his time, as it comes long before methodologists made appeals to estimate and plot dynamic quantities of interest (De Boef and Keele, 2008; Williams and Whitten, 2012). He discusses the Instantaneous Treatment Effect estimates directly and notes that it is consistent with his hypothesis: "the symmetric model suggests that such a shift leads to a concurrent increase of about one negative article... The story is quite different using a model that allows for asymmetry: the concurrent

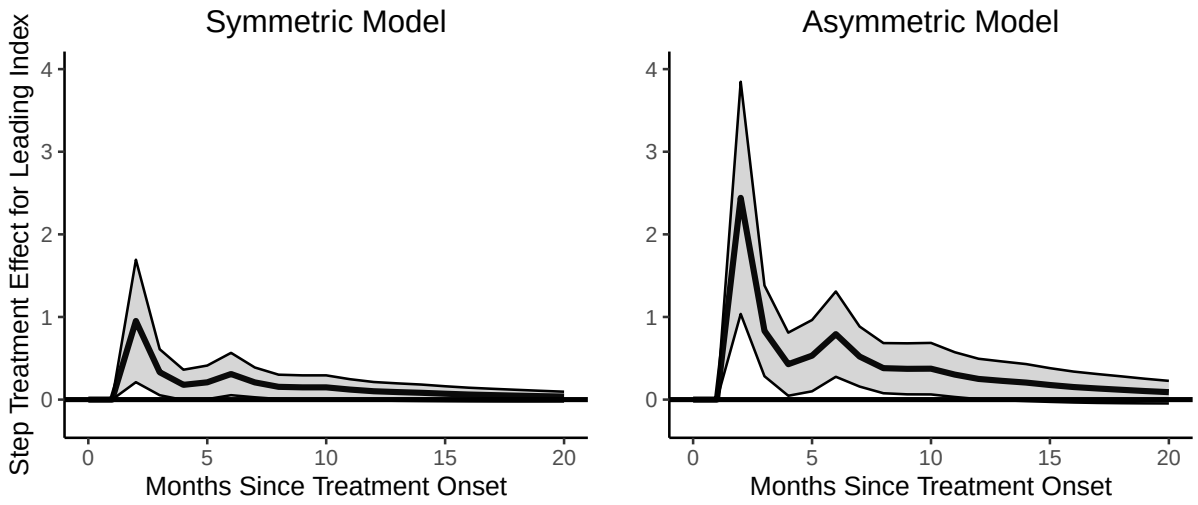

Figure 3: Estimates of the Step Treatment Effect of a Permanent, Standard Deviation Worsening of the Leading Index on the Number of Articles in *The Times* Negatively Describing the U.K. Economy, 1986-2000 (by Model)

effect of a negative shift increases to about three additional articles" (p. 381). He also notes that the effects decay to zero over time.

Because the independent variable is first-differenced, the Pulse Treatment Effect for the differenced independent variable is equivalent to the Step Treatment Effect for the independent variable in levels. And we ultimately agree that the Step Treatment Effect is a good choice for the Leading Index in levels: because the series is integrated, any change to the Leading Index is permanently incorporated into the series. As such, a permanent one standard deviation increase to the series is a perfectly reasonable counterfactual treatment. For the same reason, a Pulse Treatment Effect would not be reasonable because the only way for an integrated series to experience that would be to suffer two equal shocks in the opposite direction at consecutive time points. A linear trend treatment effect could be considered, but examination of the results from the Dickey-Fuller test on the independent variable do not give any indication that the variable has a drift or trend.

Re-analyzing these data with our GDTE framework would resolve many problems. First, his calculations are clearly approximations because they do not reflect the notable fourth-

order autoregression in the dependent variable. Second, his figure does not make it clear when treatment starts and thus when each of these effects can be expected to occur. This is especially problematic because the independent variable only enters as a second lag in the model, with no contemporaneous observation or first lag. Finally, there are no measures of uncertainty in his effect estimates. All of these issues can be solved by calculating dynamic causal estimates for dynamic linear models using the `tseffects` package in `R`.

The results of these calculations are plotted in Figure 3. These plots reveal that there is no short-run effect of the leading index on negative news coverage of the economy. This is because the independent variable only enters as a second lag and, therefore, the Instantaneous Treatment Effect is assumed to be zero. The first statistically significant effect occurs two months after a counterfactual step treatment begins, consistent with the assumptions of the models. But the effect sizes are more modest than the ones reported by Soroka: a permanent, standard deviation worsening of the economy only causes an additional 2.5 negative news stories two periods after treatment onset in the asymmetric model. As time passes since the treatment began, the effect estimates decay towards zero. But there is a notable exception six periods after treatment; a permanent, standard deviation worsening of the economy causes an additional 0.8 negative news stories six periods after treatment onset in the asymmetric model, doubtlessly a result of the strong seasonal autocorrelation in this monthly data. After this, effects decay again until they are indistinguishable from zero. Finally, to emphasize our contribution, the GDTE allows us to estimate these effects in levels, regardless of the form of the independent variable, with minimal burden on the user beyond a well-specified model.

8 Discussion and Conclusion

Integration poses a serious challenge for drawing inferences from time series and time series cross-sectional data. Scholars using dynamic regression models have been limited in their

inferences as a result of the differencing required to consistently estimate such a model, while the problem of unit roots has largely been ignored when discussing other research designs. We offer a unified way forward—the Generalized Dynamic Treatment Effect—that is agnostic to research design but allows the calculation of treatment effects in levels even if an identification assumption requires variables to be in differences. We also demonstrate that this new dynamic causal estimand is particularly adept at obtaining causal inferences in levels from models estimated in differences for the most common of such dynamic linear models, the ADL and GECM. Finally, we illustrate this usefulness with an empirical replication.

We make the results of our analysis accessible to scholars via the `tseffects` package, which calculates the Generalized Dynamic Treatment Effect from dynamic regression models for any amount of differencing of the dependent and independent variables. While certainly a boon to those who use such models, additional computational work must be done to ensure the results in this paper can be used by those who favor alternative research designs. With a differenced dependent variable, practitioners will have to find methods to calculate sums of dynamic causal estimates and, more importantly, to calculate measures of uncertainty for such sums. With a differenced independent variable, they must figure out how to calculate the Generalized Dynamic Treatment Effect more generally. We think such work is best left to those who champion the use of such research designs.

While we advance the use of the dynamic regression model for dynamic causal inference, there is still work that needs to be done to make sure the contribution fits in with other recent efforts to tackle effect heterogeneity. Warner, Vande Kamp and Jordan (Forthcoming) propose a general interactive specification for dynamic linear models that feature the use of cross-lags, $x_{t-i}z_{t-j}$. While cross-lags do not pose a problem to our results generally, they would introduce problems to the calculation of the Generalized Dynamic Treatment Effect from dynamic regression models if the cross-lags are used to allow the efficacy of treatment to change based on prior treatment history, $x_t x_{t-i}$. Additional work would need to be done

to ensure that the Generalized Dynamic Treatment Effect could still be calculated in such circumstances; we leave such an effort to future work.

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