Online Supplementary Appendix of
"A Distributional Synthetic Control Method for Policy Evaluation"

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This appendix includes a comparison between our method and certain existing methods, more discussions about the MW case studies, a revisit to the case study on CTCP considered by ADH (2010) and a mathematical proof of Proposition 1. Tables A.1-A.4 and Figures A.1-A.12, mentioned in the paper, are also presented here.

1 Comparison with existing methods

In applications, one might consider applying the time-difference method, the comparative-case-study method and the standard difference-in-differences method to the distributional context of interest to us, though these existing methods are originally established in a two-dimensional context for evaluating the mean effect. Given model (1), conditional on the actual outcome of D_{it} in (2), we may also compare the data requirements of these methods in generating an unbiased estimator for the τ-quantile intervention effect.

Time difference

Model (1) implies the time difference for the treated unit:

$$\Delta^q_{1t}(\tau) = \Delta^g_t(\tau) + \alpha_1^\top(\tau)\Delta^h_t + x_1^\top\Delta^\beta_t(\tau) + \gamma_1^\top(\tau)\Delta^z_{1t} + \delta_t(\tau) + \Delta^\varepsilon_{1t}(\tau) \tag{A1}$$

for $t \geq T_o + 1$, in which $\Delta^q_{it}(\tau) := q_{it}(\tau) - q_{iT_o}(\tau)$, $\Delta^g_t(\tau) := g_t(\tau) - g_{T_o}(\tau)$, $\Delta^h_t := h_t - h_{T_o}$, $\Delta^\beta_t(\tau) := \beta_t(\tau) - \beta_{T_o}(\tau)$, $\Delta^z_{it} := z_{it} - z_{i,T_o}$ and $\Delta^\varepsilon_{it}(\tau) := \varepsilon_{it}(\tau) - \varepsilon_{iT_o}(\tau)$ for $i = 1$. Because $\mathbb{E}[\Delta^\varepsilon_{it}(\tau)] = 0$, the time-difference estimator $\Delta^q_{1t}(\tau)$ is unbiased for the τ-quantile intervention effect $\delta_t(\tau)$ under the following condition:

$$\Delta^g_t(\tau) = 0, \quad \Delta^h_t = 0, \quad \Delta^\beta_t(\tau) = 0 \quad \text{and} \quad \Delta^z_{1t} = 0. \tag{A2}$$

This condition requires that there is no time-varying factors (or effects) in addition to the intervention effect, regardless of whether the factors (or effects) are observable or latent. This requirement is obviously too restrictive in the time-series context. Without this condition, the first-difference estimator $\Delta^q_{1t}(\tau)$ is in general biased for $\delta_t(\tau)$.

Comparative case study

Model (1) also implies the cross-sectional difference between the treated unit and a selected control unit (for some $i > 1$):

$$\Delta^q_{1i,t}(\tau) = \Delta^{\alpha\top}_{1i}(\tau)h_t + \Delta^{x\top}_{1i}\beta_t(\tau) + \Delta^{\gamma\top}_{1i}(\tau)z_{1t} + \gamma_i^\top(\tau)\Delta^z_{1i,t} + \delta_t(\tau) + \Delta^\varepsilon_{1i,t}(\tau) \tag{A3}$$

for $t \geq T_o + 1$, in which $\Delta^q_{1i,t}(\tau) := q_{1t}(\tau) - q_{it}(\tau)$, $\Delta^\alpha_{1i}(\tau) := \alpha_1(\tau) - \alpha_i(\tau)$, $\Delta^x_{1i} := x_1 - x_i$, $\Delta^\gamma_{1i}(\tau) := \gamma_1(\tau) - \gamma_i(\tau)$, $\Delta^z_{1i,t} := z_{1t} - z_{it}$ and $\Delta^\varepsilon_{1i,t}(\tau) := \varepsilon_{1t}(\tau) - \varepsilon_{it}(\tau)$. Because $\mathbb{E}[\Delta^\varepsilon_{1i,t}(\tau)] = 0$, the cross-sectional-difference estimator $\Delta^q_{1i,t}(\tau)$ is unbiased for $\delta_t(\tau)$ under the following condition:

$$\Delta^\alpha_{1i}(\tau) = 0, \quad \Delta^x_{1i} = 0, \quad \Delta^\gamma_{1i}(\tau) = 0 \quad \text{and} \quad \Delta^z_{1i,t} = 0. \tag{A4}$$

This condition requires the selected control unit to be comparable with the treated unit in the sense that there is no heterogeneity among their observable or latent features, regardless of whether the features are static or dynamic. However, it may be difficult to find out a comparable control unit that satisfies condition (A4) in practice.

Difference in differences

Similar to (A1), model (1) also implies the time-difference for a selected control unit ($i > 1$):

$$\Delta^q_{it}(\tau) = \Delta^g_t(\tau) + \alpha_i^\top(\tau)\Delta^h_t + x_i^\top\Delta^\beta_t(\tau) + \gamma_i^\top(\tau)\Delta^z_{it} + \Delta^\varepsilon_{it}(\tau) \tag{A5}$$

for $t \geq T_o + 1$. By subtracting (A5) from (A1), we obtain that

$$\nabla^q_{1i,t}(\tau) = \Delta^{\alpha\top}_{1i}(\tau)\Delta^h_t + \Delta^{x\top}_{1i}\Delta^\beta_t(\tau) + \Delta^{\gamma\top}_{1i}(\tau)\Delta^z_{1t} + \gamma_i^\top(\tau)\nabla^z_{1i,t} + \delta_t(\tau) + \nabla^\varepsilon_{1i,t}(\tau) \tag{A6}$$

holds for $t \geq T_o + 1$, in which $\nabla^z_{1i,t}(\tau) := \Delta^z_{1t} - \Delta^z_{it}$, $\nabla^\varepsilon_{1i,t}(\tau) := \Delta^\varepsilon_{1t}(\tau) - \Delta^\varepsilon_{it}(\tau)$ and

$$\nabla^q_{1i,t}(\tau) := \Delta^q_{1t}(\tau) - \Delta^q_{it}(\tau) = \Delta^q_{1i,t}(\tau) - \Delta^q_{1i,T_o}(\tau). \tag{A7}$$

The first equality in (A7) defines the difference-in-differences estimator $\nabla^q_{1i,t}(\tau)$ by first taking the unit-specific time differences and then taking the cross-sectional difference of the time differences. The second equality in (A7) shows that this estimator can also be defined by first taking a cross-sectional difference and then taking the time difference of the cross-sectional difference sequence. Because $\mathbb{E}[\nabla^\varepsilon_{1i,t}(\tau)] = 0$, the estimator $\nabla^q_{1i,t}(\tau)$ is unbiased for $\delta_t(\tau)$ under the following condition:

$$\begin{gathered}\left(\Delta^\alpha_{1i}(\tau) = 0 \text{ or } \Delta^h_t = 0\right), \quad \left(\Delta^x_{1i} = 0 \text{ or } \Delta^\beta_t(\tau) = 0\right), \quad (\Delta^\gamma_{1i}(\tau) = 0 \text{ or } \Delta^z_{1t} = 0) \\ \text{and } \left(\gamma_i(\tau) = 0 \text{ or } \nabla^z_{1i,t} = 0\right),\end{gathered} \tag{A8}$$

for $i > 1$. Compared to condition (A2), condition (A8) is weaker by allowing for a latent time-varying common factor: $\Delta^g_t(\tau) \neq 0$ provided that $\Delta^z_{it} = \Delta^z_{1t} = 0$ (which implies $\nabla^z_{1i,t} = 0$) for $i > 1$. In addition, compared to condition (A4), condition (A8) allows for the presence of fixed effects: $\Delta^\gamma_{1i}(\tau) \neq 0$ and the static heterogeneity: $\Delta^x_{1i} \neq 0$ provided that $\Delta^z_{1t} = 0$

and $\Delta_t^{\beta}(\tau) = 0$. It is easy to see that condition (A8) is satisfied for the two-way fixed-effects model:

$$q_{it}(\tau) = g_t(\tau) + \gamma_i(\tau) + x_i^{\top}\beta(\tau) + \delta_t(\tau)D_{it} + \varepsilon_{it}(\tau), \tag{A9}$$

which is a a particular case of model (1) with the "time-specific fixed effect" $g_t(\tau)$, the "unit-specific fixed effect" $\gamma_i(\tau)$ and the time-invariant coefficient $\beta(\tau)$. According to the two-way fixed-effects model, we can observe that the difference-in-differences method requires the treated and control units to share the parallel time trends: $g_t(\tau) + \gamma_i(\tau)$ for $i = 1$ and $i > 1$. In general, this requirement is rather restrictive because it precludes the treated and control units from having heterogeneous time trends. In comparison, model (1) allows for the non-parallel time trends caused by the presence of any of the components: $\alpha_i^{\top}(\tau)h_t$, $x_i^{\top}\beta_t(\tau)$ and $\gamma_i^{\top}(\tau)z_{it}$ under Assumption 1.

2 Case studies on MW hikes

2.1 A review on the empirical MW literature

The federal (hourly) MW in the U.S. was initiated by the Fair Labor Standards Act of 1938 in order to maintain a basic living standard for workers. In addition to the federal MW, each state may set a higher MW by legislation. The effective MW for most workers is the maximum of the federal MW and the state-level MW (the federal MW) for a state with (without) its own state-level MW, while subminimum wages may also be provided for certain groups of workers. Although the MW policies are proposed to protect the basic well-being of low-wage workers, the literature lacks a consensus regarding whether the intervention effects of MW are consistent with this policy target. In particular, as predicted by a simple labor supply-and-demand model, earlier time-series studies have shown that the increase in MW might cause disemployment effects for low-wage workers. However, since the 1990s, several case studies have indicated that the increase in MW might not cause disemployment effects; see, e.g., Brown (1999) for a review on the earlier MW literature and Card and Krueger (2016) for such a collection of case studies and related discussions.

The current development of MW studies is greatly influenced by a special issue on "New Minimum Wage Research" published by the *Industrial and Labor Relations Review* in 1992, as mentioned by Neumark et al. (2014, p.609). Since then, it has become a standard to use state-level, or even finer-level, panel data, rather than national-level time series data, to investigate the intervention effects of MW. For instance, Neumark and Wascher (1992) used two-way fixed-effects models and a national state-level panel data to estimate the (dis)employment effects of MW hikes. Card (1992) conducted a case study that evaluated the intervention effects of the 1988 California MW hike on the earnings and employment of the treated state's teenagers. Later, Card and Krueger (1994) conducted an influential case study to evaluate the intervention effects of the 1992 New Jersey MW hike on the treated state's fast-food industry using telephone survey data; see also Card and Krueger (2000) and Neumark and Wascher (2000) for case studies on the same event using other types of data and Dube et al. (2007) for a case study on the intervention effects of San Francisco's citywide MW hikes in 2004 and in 2007. It can be observed from this literature that the disagreement regarding the intervention effects of MW may be closely related to the differences in the econometric methods being used to identify and estimate the effects in addition to various types of data.

Although the use of a fixed-effects model amounts to pooling a set of individual case studies in the same regression context, as mentioned by Dube, Lester and Reich (2010, DLR), the aforementioned two-way fixed-effects model and case studies both make use of the difference-in-differences method in identifying the intervention effects of MW hikes. Thus, it is especially important to assess the appropriateness of the control units underlying these methods by examining whether the parallel-trend assumption holds for the treated and control units. DLR pointed out that this assumption may fail when the two-way fixed-effects model is directly applied to national state-level panel data without accounting for the time-varying spatial heterogeneity of the treated and control states. In comparison, Card and Krueger (1994) accounted for this issue by selecting Eastern Pennsylvania as the control unit for New Jersey in their case study. This control unit is geographically connected to and economically similar to the treated state, and is free of a state-level MW hike in the sampling period. Similarly, Dube et al. (2007) chose the San Francisco Bay metropolitan area besides San Francisco itself as the control unit for San Francisco to evaluate the intervention effects of San Francisco's citywide MW hikes. Such geographically local controls are explicitly or implicitly selected to remedy the possible failure of the parallel-trend assumption caused by the time-varying spatial heterogeneity. DLR further extended this notion to a county-level fixed-effects model that attempts to alleviate the time-varying spatial heterogeneity by setting the border counties of an untreated state as the control units of the contiguous counties of a treated state.

As discussed in the Introduction, the choice of an appropriate control unit is undoubtedly essential for establishing a suitable counterfactual of the treated unit in policy evaluation. There is a growing interest in applying the synthetic control method of ADH (2010) to the MW studies; see, e.g., Sabia et al. (2012) and Reich et al. (2017). This development is largely motivated by the advantages of this method in replacing a judgmental selection of control unit with a data-driven control unit which is established in a more transparent and systematic way. Moreover, the optimal combination weights obtained by this method allow researchers to reexamine the validity of pre-selected control units like the aforementioned geographically local controls; see, e.g., Neumark and Wascher (2017) and Allegretto et al. (2017). Nonetheless, as mentioned, the conventional synthetic-control method may fail to generate an appropriate counterfactual in the presence of poor matching; see also Allegretto et al. (2017) for related discussions. In addition, an increase in MW may have heterogeneous intervention effects on various subunits (individuals) of the treated unit. This might also constitute a part of the reasons underlying the disagreement regarding the literature's empirical findings. Thus, it is important to explore the distributional effects in order to understand the influences of MW policies in a more complete way.

2.2 Empirical analysis

2.2.1 Minimum data requirement

Figure A.8, we show the number of counties with complete data in the sampling period. Texas (Delaware) is the largest (smallest) state that has 103 counties (2 counties) with complete data. The minimum data requirement: $n_{it} > 20$, for all t's, precludes 17 states from our empirical analysis.

2.2.2 Events involving state-level MW hikes

The case studies to be explored are identified from the data for MWs and outcome variables for the 31 states that satisfy the minimum data requirement. Figure A.11 shows the time series of the federal MW which is common to all the states, and plots the time series of the state-level MW which is fixed at zero for the states without their own MWs or that varies over time for the states with their own MWs. Among the 31 states, there are 21 states (Alabama, Arkansas, Colorado, Georgia, Indiana, Kansas, Kentucky, Louisiana, Michigan, Mississippi, Missouri, Nebraska, North Carolina, Ohio, Oklahoma, Pennsylvania, South Carolina, Tennessee, Texas, Virginia and West Virginia) that have no state-level MWs in the sampling period. For these states, the effective MW is the same as the federal MW in the sampling period. In comparison, there are 10 states (California, Florida, Illinois, Iowa, Maryland, Minnesota, New York, Oregon, Washington and Wisconsin) that have increased their state-level MWs on at least one occasion during the sampling period. For these states, the effective MW is the maximum of the federal MW and the state-level MW, and there are a total of 32 events involving state-level MW hikes in the sampling period.

For an event involving a state-level MW hike, we define the pre-intervention period (the post-intervention period) as the subperiod before this intervention (the next intervention) and after the previous intervention (this intervention). Table A.3 summarizes the state-level MWs in the pre-intervention and post-intervention periods of these 32 events, the federal and effective MWs at the T_o's and in the post-intervention periods, and the associated state-level and effective MW changes.

We require that the pre-intervention duration not be shorter than 20 quarters (that is, $T_o \geq 20$). This requirement reduces the 32 events to the following nine events: the state-level MW hikes of California in 1997:Q1, Florida in 2005:Q2, Illinois in 2004:Q1, Maryland in 2006:Q1, Minnesota in 2005:Q3, New York in 2005:Q1, Oregon in 1997:Q1, Washington in 1999:Q1 and Wisconsin in 2005:Q2.

2.2.3 Outliers

Among the aforementioned nine events, the time series of labor earnings show obvious outliers for Florida, Illinois, Maryland, Minnesota and New York. Let $\hat{q}_{it}(\tau)$ be the sample τ-quantile of the i-th state's labor earnings. An outlier is detected for the i-th state if $|\hat{q}_{i,t+1}(\tau) - \hat{q}_{it}(\tau)|$, or $|\hat{q}_{it}(\tau) - \hat{q}_{i,t-1}(\tau)|$, exceeds \$100 for some τ and for some t in 1990Q1-2006Q2. The maximum outliers are \$146.875 for Florida in 1994Q3, \$168.827 for Illinois in 1993Q2, \$459.681 for Maryland in 1990Q1, \$743.966 for Minnesota in 1991Q2 and \$444.135 for New York in 1991Q3.

The donor pools of the four case studies are also determined by precluding the potential control states with outliers. For Case 1 and Case 2, we preclude Florida, Georgia, Illinois, Maryland, Nebraska and New York from the donor pools. For Case 3, we preclude Florida, Georgia, Minnesota, Nebraska, Oklahoma and Texas from the donor pool. For Case 4, we preclude Colorado, Kentucky, Nebraska, Tennessee and Texas from the donor pool. These states are precluded because their labor earnings show obvious outliers in the pre-intervention periods such that $|\hat{q}_{i,t+1}(\tau) - \hat{q}_{it}(\tau)|$, or $|\hat{q}_{it}(\tau) - \hat{q}_{i,t-1}(\tau)|$, exceeds \$100 for some τ and for some $t \in [1, T]$.

2.2.4 Economic features

Table A.4 reports the observable economic features of the treated state with the counterparts of the average synthetic-control state and the τ-quantile synthetic-control states for $\tau = 0.1$, 0.5 and 0.9. It also reports the economic features of a single-best control state in the donor pool that are closest to their treated counterparts in terms of the Euclidean norm. The single-best control state is Colorado for Case 1 and Case 2, Missouri for Case 3 and Michigan for Case 4. From Figure 2 of the paper, we can observe that for labor earnings, the contributions of the single-best control states to the average synthetic-control states are approximately zero for Cases 1 and 3, 0.18 for Case 2 and 0.02 for Case 4, respectively. Obviously, the single-best control states are substantially dominated by the synthetic-control states in matching the pre-intervention features and outcomes of the treated state. Indeed, Table A.4 suggests that the synthetic-control states in general outperform the single-best control states in approximating the treated state's economic features. The synthetic-control states also reasonably mimic the treated states in most cases. However, for Case 1, the synthetic California obviously underestimates the population and the non-farm employment (overestimates the land area) of California. This exception is sensible because California (Texas which is the main component of the synthetic California) is the largest state in terms of population (land area) in the U.S. The aforementioned results in general hold regardless of whether we replace the average synthetic-control state with a quantile synthetic-control state or replace the synthetic-control state for labor earnings with its counterpart for employment.

2.2.5 Mean effects

Table A.5 summarizes the mean effects, the elasticities of the effects and the p-values of the associated placebo tests. The associated sample means at T_o and in the post-intervention period are also reported. In this table, the elasticity of a mean effect is defined as the ratio of the percentage change in the mean effect over the percentage change in the treated state's effective wage:

$$e_{\mu,t} := \left(\frac{\hat{\mu}_{1t} - \hat{\hat{\mu}}_{1t}^{(0)}}{\hat{\hat{\mu}}_{1t}^{(0)}}\right) \Big/ \left(\frac{EMW_{T_o+1} - EMW_{T_o}}{EMW_{T_o}}\right)$$

for $t \geq T_o + 1$, where EMW_t is the effective MW at $t = T_o$ or $T_o + 1$. From this table, we can observe that the p-values of the mean effects on labor earnings (employment) are, respectively, 0.857 and 0.353 (0.619 and 0.471) for Case 1 and Case 4. Obviously, the estimates of the mean effects are insignificant for these two cases. This suggests that the increase in California's (effective) hourly MW by \$0.75 (by \$0.25) in 1997Q1 might have no statistically significant influence on the mean of the average weekly earnings of restaurant workers and the mean of the total restaurant employment for California within the post-intervention period 1997Q1-1997Q2. Similarly, the increase in the state-level (effective) hourly MW by \$2.05 (by \$0.55) in Wisconsin in 2005Q2 might also have no statistically significant influence on the means of labor earnings and employment for the state's restaurant industry within the post-intervention period 2005Q2-2006Q2.

In comparison, the p-values of the mean effects on labor earnings (employment) are, respectively, 0.048 and 0.043 (0.048 and 0.087) for Case 2 and Case 3. This shows that the estimates of the mean effects on labor earnings are significant at the 5% level for these two cases, and the estimates of the mean effects on employment are significant at the 5% level for Case 2 and at the 10% level for Case 3. According to the estimates presented in Table 5, the increase in the state-level (effective) hourly MW by \$0.75 (by \$0.75) in Oregon in 1997Q1 might cause the increase in the mean of the average weekly earnings of restaurant workers of the state by \$3.475 in 1997Q1, \$6.787 in 1997Q2, \$2.138 in 1997Q3 and \$4.734 in 1997Q4 at the cost of decreasing the mean of the total restaurant employment level of the state by 56 in 1997Q1, 76 in 1997Q2, 144 in 1997Q3 and 156 in 1997Q4. These mean effects on labor earnings (employment) are, respectively, of the elasticities: 0.138, 0.259, 0.076 and 0.173 (-0.124, -0.159, -0.287 and -0.320). In addition, the increase in the state-level (effective) hourly MW by \$0.80 (by \$0.55) in Washington in 1999Q1 might cause the increase in the mean of the average weekly earnings of restaurant workers of the state by \$4.151 in 1999Q1, \$7.664 in 1999Q2, \$7.867 in 1999Q3 and \$7.583 in 1999Q4 at the cost of decreasing the mean of the total restaurant employment level of the state by 70 in 1999Q1, 85 in 1999Q2, 142 in 1999Q3 and 230 in 1999Q4. These mean effects on labor earnings (employment) are, respectively, of the elasticities: 0.238, 0.416, 0.404 and 0.395 (-0.175, -0.205, -0.333 and -0.534). This shows that Case 2 and Case 3 share a similar pattern of the mean effects of MW hikes.

2.2.6 Quantile effects

Table A.5 also shows the estimates of the quantile effects $\hat{\hat{\delta}}_t(\tau)$'s, the associated elasticities $e_t(\tau)$'s and the p-values of the placebo tests for $\tau = 0.1$, 0.5 and 0.9, in which the elasticity of the τ-quantile effect is defined by using $(\hat{q}_{1t}(\tau), \hat{\hat{q}}_{1t}^{(0)}(\tau))$ in place of the role of $(\hat{\mu}_{1t}, \hat{\hat{\mu}}_{1t}^{(0)})$ in $e_{\mu,t}$. The associated sample quantiles at T_o and in the post-intervention period are also reported.

Focusing on Case 2 and Case 3, the estimates shown in Table A.5 suggest that the increase in the state-level (effective) hourly MW by \$0.75 (by \$0.75) in Oregon in 1997Q1 might cause the increase in the τ-quantile of the average weekly earnings of restaurant workers of the state in 1997Q1 by \$6.684 for $\tau = 0.1$, \$2.420 for $\tau = 0.5$ and \$1.356 for $\tau = 0.9$ at the cost of decreasing the τ-quantile of the total restaurant employment level of the state in 1997Q1 by 14 for $\tau = 0.1$, 37 for $\tau = 0.5$ and 255 in 1997Q4 for $\tau = 0.9$. In comparison, the increase in the state-level (effective) hourly MW by \$0.80 (by \$0.55) in Washington in 1999Q1 might cause the increase in the τ-quantile of the average weekly earnings of restaurant workers of the state in 1999Q1 by \$2.659 for $\tau = 0.1$, \$3.245 for $\tau = 0.5$ and \$5.317 for $\tau = 0.9$ and the increase (decrease) of the τ-quantile of the total restaurant employment level of the state in 1999Q1 by 31 for $\tau = 0.1$ (by 40 for $\tau = 0.5$ and 204 for $\tau = 0.9$). Although these two cases share a similar pattern of the mean effects, this comparison illustrates that their quantile effects are quite different. The increase in a state-level MW might cause greater (smaller) positive impacts on labor earnings for low quantiles than for high quantiles at $T_o + 1$ for Case 2 (Case 3), and might cause greater negative impacts on employment for high quantiles in both cases. As shown in Figure 7 of the paper, the impacts also tend to change over time in the post-intervention period.

Corresponding to Figure 7 of the paper, we plot the p-values of the placebo tests in Figure A.13 for the whole range of τ. It shows that, like the mean effects on labor earnings, the estimates of the quantile effects on labor earnings are in general insignificant at the 10% level for Case 1 and Case 4 with the exception of a few τ's. In comparison, the estimates of the quantile effects on labor earnings are significant at the 5% level for certain ranges of τ's for Case 2 and Case 3, and the estimates of the quantile effects on employment are in general insignificant at the 10% level for most τ's. Thus, the significance of the intervention effects tends to be event-specific.

3 Case study on CTCP

We also apply the proposed method to revisit the case study considered by ADH. In this case study, the policy intervention is California's Proposition 99, referred to as the CTCP here, that was passed in November 1988 and implemented in January 1989. The outcome variable is the yearly per capita cigarette sales in California (the treated state). The sampling period is composed of the pre-intervention period: 1970-1988 and the post-intervention period: 1989-2000. To implement their synthetic control analysis, ADH considered a donor pool of 38 potential control states by excluding the District of Columbia and the eleven states that implemented some large-scale tobacco control programs or raised the state cigarette taxes by at least 50 cents in the post-intervention period. In addition, they considered a seven-dimensional x_i ($r = 7$) to represent the observable static characteristics of the i-th state for $i = 1, 2, \ldots, N$ and $N = 39$; see ADH (Table 1). We download the state-level data of the per capita cigarette sales and the x_i's of ADH from `http://fmwww.bc.edu/repec/bocode/s/synth_smoking.dta`. The time series of the per capita cigarette sales of California and the

38 potential control states are shown in Figure A.14. Before further discussions, it should be noted that we may only evaluate the mean effects of CTCP on California's cigarette sales in this case study because of data restriction. As shown by Figure A.14, we only observe the state-level data of the outcome variable in this empirical context.

As mentioned in Section 2 of the paper, the distributional synthetic-control analysis is built on a set of subunit-level (or individual-level) data of the outcome variable. Nonetheless, we may still compare the proposed method with the conventional synthetic-control method in terms of evaluating the mean effects. In this scenario, the three-dimensional fixed-effects factor model presented in Equation (1) of the paper reduces to the two-dimensional model shown in Equation (15) of the paper, and the proposed method is based on the latter for evaluating the mean effects. Our method deals with the potential poor-matching problem by accounting for the observed dynamic heterogeneity among the treated and potential control states. In comparison, the conventional method (conducted by ADH) is based on a special case of this two-dimensional model where the observed dynamic-heterogeneity component $\alpha_i^\top h_t$ is absent (and $z_{it} = z_t$). In this case study, we set $h_t = (1, t, t^2)^\top$ in order to capture the observed dynamic heterogeneity among the cigarette-sale time series of the treated and potential control states in levels and trends. This type of heterogeneity is observed from Figure A.14.

Corresponding to Equation (23) of the paper, we compute the weighting vector of the proposed method as the solution to a quadratic-programming problem:

$$\hat{\boldsymbol{w}}_\mu^o := \underset{\boldsymbol{w}\in\mathbb{W}}{\operatorname{argmin}} \left(\hat{\boldsymbol{\psi}}^* - \hat{\boldsymbol{\Psi}}^* \boldsymbol{w}\right)^\top \left(\hat{\boldsymbol{\psi}}^* - \hat{\boldsymbol{\Psi}}^* \boldsymbol{w}\right),$$

where $\hat{\boldsymbol{\psi}}^*$ is a $(r + T_o) \times 1$ vector that comprises x_1 and the pre-intervention least-squares residuals obtained by regressing the treated state's per capita cigarette sales on h_t, and $\hat{\boldsymbol{\Psi}}^*$ is a $(r + T_o) \times (N - 1)$ matrix that comprises the $(N - 1)$ potential-control counterparts of $\hat{\boldsymbol{\psi}}^*$. In addition, we compute the weighting vector of the conventional method as the solution to another quadratic-programming problem:

$$\hat{\boldsymbol{w}}_\mu^\dagger := \underset{\boldsymbol{w}\in\mathbb{W}}{\operatorname{argmin}} \left(\hat{\boldsymbol{\psi}} - \hat{\boldsymbol{\Psi}} \boldsymbol{w}\right)^\top \left(\hat{\boldsymbol{\psi}} - \hat{\boldsymbol{\Psi}} \boldsymbol{w}\right),$$

where $\hat{\boldsymbol{\psi}}$ is a $(r + T_o) \times 1$ vector that comprises x_1 and the treated state's pre-intervention per capita cigarette sales, and $\hat{\boldsymbol{\Psi}}$ is a $(r + T_o) \times (N - 1)$ matrix that comprises the $(N - 1)$ potential-control counterparts of $\hat{\boldsymbol{\psi}}$. Moreover, we let $\hat{\boldsymbol{w}}_\mu^\ddagger$ be the weighting vector of the conventional method reported by ADH (Table 2). Note that the only difference between $\hat{\boldsymbol{w}}_\mu^\dagger$ and $\hat{\boldsymbol{w}}_\mu^\ddagger$ is that $\hat{\boldsymbol{w}}_\mu^\ddagger$ is computed using the two-step optimization method of Abadie and Gardeazabal (2003, Appendix B). As will be shown later, the synthetic-control state (synthetic California) generated by $\hat{\boldsymbol{w}}_\mu^\dagger$ is almost the same as its counterpart generated by $\hat{\boldsymbol{w}}_\mu^\ddagger$.

In Figure A.15, we show the time series of per capita cigarette sales of California and the main control states underlying the synthetic California generated by $\hat{\boldsymbol{w}}_\mu^o$, and report the associated combination weights and time series of these control states. Figure A.16 is the counterpart of Figure A.15 generated by $\hat{\boldsymbol{w}}_\mu^\ddagger$. By comparing these two figures, we can observe that the synthetic California generated by $\hat{\boldsymbol{w}}_\mu^o$ is different from that generated by $\hat{\boldsymbol{w}}_\mu^\ddagger$ in terms of their main control states and combination weights. Indeed, the pre-intervention mean squared prediction error (MSPE) is 0.633 for the $\hat{\boldsymbol{w}}_\mu^o$-based synthetic California but 3.089 for the $\hat{\boldsymbol{w}}_\mu^\ddagger$-based synthetic California, and the latter is close to the pre-intervention MSPE 2.803 for the $\hat{\boldsymbol{w}}_\mu^\dagger$-based synthetic California. This shows that the proposed method outperforms the conventional method (of ADH) in matching the static characteristics and the pre-intervention time series of per capita cigarette sales of California and their counterpart of the synthetic California.

In Figure A.17, we further compare the actual time series of per capita cigarette sales of California with its counterfactuals generated by $\hat{\boldsymbol{w}}_\mu^o$, $\hat{\boldsymbol{w}}_\mu^\dagger$ and $\hat{\boldsymbol{w}}_\mu^\ddagger$ during the whole sampling period. From this figure, we can observe that the counterfactual generated by $\hat{\boldsymbol{w}}_\mu^\dagger$ is almost identical to that generated by $\hat{\boldsymbol{w}}_\mu^\ddagger$ during the whole sampling period, as mentioned previously. In addition, the counterfactual generated by $\hat{\boldsymbol{w}}_\mu^o$ is visually very close to that generated by $\hat{\boldsymbol{w}}_\mu^\ddagger$ during the pre-intervention period, while the former has a smaller MSPE relative to the latter. In the post-intervention period, the counterfactual generated by $\hat{\boldsymbol{w}}_\mu^o$ is higher than that generated by $\hat{\boldsymbol{w}}_\mu^\ddagger$ to some extent. Recall that the post-intervention difference between the actual time series and a counterfactual time series estimates the mean effects of intervention. This means that the proposed method attains the same empirical finding as the conventional method regarding the effectiveness of CTCP on reducing California's per capita cigarette sales, while the proposed method shows somewhat stronger effects in comparison with the conventional method.

4 Proof of Proposition 1

Given (2), model (1) implies that

$$\begin{aligned}\left[\sum_{t=1}^{T_o} q_{it}(\tau)h_t^\top\right] = \left[\sum_{t=1}^{T_o} g_t(\tau)h_t^\top\right] + \alpha_i^\top(\tau)\left[\sum_{t=1}^{T_o} h_t h_t^\top\right] + x_i^\top\left[\sum_{t=1}^{T_o} \beta_t(\tau)h_t^\top\right] \\ + \gamma_i^\top(\tau)\left[\sum_{t=1}^{T_o} z_{it}h_t^\top\right] + \left[\sum_{t=1}^{T_o} \varepsilon_{it}(\tau)h_t^\top\right],\end{aligned} \tag{A10}$$

for all i's. Under Assumption 1(i), we can use (A10) to obtain

$$\begin{aligned}&\left[\sum_{t=1}^{T_o} q_{it}(\tau)h_t^\top\right]\left[\sum_{t=1}^{T_o} h_t h_t^\top\right]^{-1} h_t = \left[\sum_{t=1}^{T_o} g_t(\tau)h_t^\top\right]\left[\sum_{t=1}^{T_o} h_t h_t^\top\right]^{-1} h_t + \alpha_i^\top(\tau)h_t \\ &+ x_i^\top\left[\sum_{t=1}^{T_o} \beta_t(\tau)h_t^\top\right]\left[\sum_{t=1}^{T_o} h_t h_t^\top\right]^{-1} h_t + \gamma_i^\top(\tau)\left[\sum_{t=1}^{T_o} z_{it}h_t^\top\right]\left[\sum_{t=1}^{T_o} h_t h_t^\top\right]^{-1} h_t \\ &+ \left[\sum_{t=1}^{T_o} \varepsilon_{it}(\tau)h_t^\top\right]\left[\sum_{t=1}^{T_o} h_t h_t^\top\right]^{-1} h_t,\end{aligned} \tag{A11}$$

for all (i,t)'s, and define the least-squares residuals:

$$g_t^*(\tau) := g_t(\tau) - \left[\sum_{t=1}^{T_o} g_t(\tau) h_t^\top\right]\left[\sum_{t=1}^{T_o} h_t h_t^\top\right]^{-1} h_t,$$

$$\beta_t^*(\tau) := \beta_t(\tau) - \left[\sum_{t=1}^{T_o} \beta_t(\tau) h_t^\top\right]\left[\sum_{t=1}^{T_o} h_t h_t^\top\right]^{-1} h_t,$$

$$z_{it}^* := z_{it} - \left[\sum_{t=1}^{T_o} z_{it} h_t^\top\right]\left[\sum_{t=1}^{T_o} h_t h_t^\top\right]^{-1} h_t$$

and

$$\varepsilon_{it}^*(\tau) := \varepsilon_{it}(\tau) - \left[\sum_{t=1}^{T_o} \varepsilon_{it}(\tau) h_t^\top\right]\left[\sum_{t=1}^{T_o} h_t h_t^\top\right]^{-1} h_t.$$

By subtracting (A11) from (1), we obtain that

$$q_{it}^*(\tau) = g_t^*(\tau) + x_i^\top \beta_t^*(\tau) + \gamma_i^\top(\tau) z_{it}^* + \delta_t(\tau) D_{it} + \varepsilon_{it}^*(\tau), \tag{A12}$$

for all (i,t)'s. Let $\boldsymbol{w}(\tau)$ be an $(N-1)\times 1$ vector in $\mathbb{W}$, and $w_i(\tau)$ be the $(i-1)$-th element of $\boldsymbol{w}(\tau)$ for $i = 2,\dots,N$. Given (2), $D_{it} = 0$ holds for all t's *if* $i > 1$. Thus, (A12) implies that

$$\sum_{i=2}^N w_i(\tau) q_{it}^*(\tau) = g_t^*(\tau) + \sum_{i=2}^N w_i(\tau) x_i^\top \beta_t^*(\tau) + \sum_{i=2}^N w_i(\tau)\gamma_i^\top(\tau) z_{it}^* + \sum_{i=2}^N w_i(\tau)\varepsilon_{it}^*(\tau). \tag{A13}$$

By subtracting (A13) from (A12) with $i = 1$, we further obtain that

$$\begin{aligned} q_{1t}^*(\tau) - \sum_{i=2}^N w_i(\tau) q_{it}^*(\tau) = {} & \left[x_1 - \sum_{i=2}^N w_i(\tau) x_i\right]^\top \beta_t^*(\tau) + \left[\gamma_1^\top(\tau) z_{1t}^* - \sum_{i=2}^N w_i(\tau)\gamma_i^\top(\tau) z_{it}^*\right] \\ & + \delta_t(\tau) D_{1t} + \left[\varepsilon_{1t}^*(\tau) - \sum_{i=2}^N w_i(\tau)\varepsilon_{it}^*(\tau)\right], \end{aligned} \tag{A14}$$

for all t's. Given (2), $D_{1t} = 0$ also holds for $t \le T_o$. Thus, (A14) means that

$$\begin{aligned} q_{1t'}^*(\tau) - \sum_{i=2}^N w_i(\tau) q_{it'}^*(\tau) = {} & \left[x_1 - \sum_{i=2}^N w_i(\tau) x_i\right]^\top \beta_{t'}^*(\tau) + \left[\gamma_1^\top(\tau) z_{1t'}^* - \sum_{i=2}^N w_i(\tau)\gamma_i^\top(\tau) z_{it'}^*\right] \\ & + \left[\varepsilon_{1t'}^*(\tau) - \sum_{i=2}^N w_i(\tau)\varepsilon_{it'}^*(\tau)\right] \end{aligned} \tag{A15}$$

for any $t' \le T_o$. By using the matching condition in Assumption 1(ii), (A15) with $\boldsymbol{w}(\tau) = \boldsymbol{w}^o(\tau)$ degenerates to the following restriction:

$$\left[\gamma_1^\top(\tau) z_{1t'}^* - \sum_{i=2}^N w_i^o(\tau)\gamma_i^\top(\tau) z_{it'}^*\right] + \left[\varepsilon_{1t'}^*(\tau) - \sum_{i=2}^N w_i^o(\tau)\varepsilon_{it'}^*(\tau)\right] = 0. \tag{A16}$$

By introducing (A16) in (A14) and using Assumption 1(ii), we obtain that

$$q_{1t}^*(\tau) - \sum_{i=2}^{N} w_i^o(\tau) q_{it}^*(\tau) = \delta_t(\tau) D_{1t} + \left[\gamma_1^\top(\tau)(z_{1t}^* - z_{1t'}^*) - \sum_{i=2}^{N} w_i^o(\tau)\gamma_i^\top(\tau)(z_{it}^* - z_{it'}^*)\right] + \left[(\varepsilon_{1t}^*(\tau) - \varepsilon_{1t'}^*(\tau)) - \sum_{i=2}^{N} w_i^o(\tau)\left(\varepsilon_{1t}^*(\tau) - \varepsilon_{1t'}^*(\tau)\right)\right],$$

for all t's. Given (8), this further implies that

$$\hat{\delta}_t(\tau) = \delta_t(\tau) + \left[\gamma_1^\top(\tau)(z_{1t}^* - z_{1t'}^*) - \sum_{i=2}^{N} w_i^o(\tau)\gamma_i^\top(\tau)(z_{it}^* - z_{it'}^*)\right] + \left[(\varepsilon_{1t}^*(\tau) - \varepsilon_{1t'}^*(\tau)) - \sum_{i=2}^{N} w_i^o(\tau)\left(\varepsilon_{1t}^*(\tau) - \varepsilon_{1t'}^*(\tau)\right)\right], \tag{A17}$$

for $t \geq T_o + 1$. Given (A17), we have $\mathbb{E}[\hat{\delta}_t(\tau)] = \delta_t(\tau)$ for $t \geq T_o + 1$ if

$$\mathbb{E}[\gamma_i^\top(\tau)(z_{it}^* - z_{it'}^*)] = 0 \tag{A18}$$

and

$$\mathbb{E}[\varepsilon_{it}^*(\tau)] = 0 \tag{A19}$$

for all (i, t, τ)'s. For $t \leq T_o$, $\hat{\delta}_t(\tau) = 0$ holds by the definition of $\hat{\delta}_t(\tau)$ in (8) under Assumption 1(ii). Thus, we can complete the proof of Proposition 1 by further showing (A18) and (A19). To show (A18), note that

$$\gamma_i^\top(\tau) z_{it}^* = \gamma_i^\top(\tau)\left(z_{it} - \left[\sum_{t=1}^{T_o} z_{it} h_t^\top\right]\left[\sum_{t=1}^{T_o} h_t h_t^\top\right]^{-1} h_t\right).$$

Under Assumption 1(iii-a), $\gamma_i(\tau)$ is independent of $\{(z_{it}^\top, h_t^\top)\}$, and hence

$$\mathbb{E}[\gamma_i^\top(\tau) z_{it}^*] = \mathbb{E}[\gamma_i^\top(\tau)]\mathbb{E}[z_{it}^*]. \tag{A20}$$

Under Assumption 1(iii-b), $\{z_{it}\}$ is independent of $\{h_t\}$, and hence

$$\mathbb{E}[z_{it}^*] = \mathbb{E}[z_{it}] - \left[\sum_{t=1}^{T_o} \mathbb{E}[z_{it}]\mathbb{E}\left[h_t^\top \left[\sum_{t=1}^{T_o} h_t h_t^\top\right]^{-1} h_t\right]\right]. \tag{A21}$$

Under Assumption 1(iii-c), $\mathbb{E}[z_{it}]$ is time-invariant, so that we can rewrite (A21) as:

$$\mathbb{E}[z_{it}^*] = \mathbb{E}[z_{it}]\mathbb{E}[\iota_t^*],$$

where

$$\iota_t^* := 1 - \left[\sum_{t=1}^{T_o} h_t^\top\right]\left[\sum_{t=1}^{T_o} h_t h_t^\top\right]^{-1} h_t.$$

Assumption 1(iii-d) implies that $\iota_t^* = 0$. Thus, $\mathbb{E}[z_{it}^*] = 0$. By introducing this result in (A20), we have $\mathbb{E}[\gamma_i^\top(\tau) z_{it}^*] = 0$ for all t's. Therefore, condition (A18) holds under Assumption 1(iii). In addition, (A19) also holds under Assumption 1(iii-b) because

$$\mathbb{E}[\varepsilon_{it}^*(\tau)] = \mathbb{E}[\varepsilon_{it}(\tau)] - \left[\sum_{t=1}^{T_o} \mathbb{E}[\varepsilon_{it}(\tau)]\mathbb{E}\left[h_t^\top \left[\sum_{t=1}^{T_o} h_t h_t^\top\right]^{-1} h_t\right]\right] = 0,$$

where the first equality is due to the condition that $\varepsilon_{it}(\tau)$ is independent of $\{h_t\}$, and the second equality is due to the condition $\mathbb{E}[\varepsilon_{it}(\tau)] = 0$. Thus, the proof of Proposition 1 is completed. $\square$

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Table A.1: Simulated averages of the pre-intervention RMSPEs.

		conventional method								proposed method							
		$T_o = 50$				$T_o = 100$				$T_o = 50$				$T_o = 100$			
		$N = 50$		$N = 100$		$N = 50$		$N = 100$		$N = 50$		$N = 100$		$N = 50$		$N = 100$	
	τ	$n = 50$	100	50	100	50	100	50	100	$n = 50$	100	50	100	50	100	50	100
Case 1	0.01	0.977	0.860	0.918	0.826	1.021	1.005	1.041	0.972	0.843	0.825	0.814	0.787	0.969	0.942	0.936	0.889
	0.05	1.030	0.887	0.969	0.857	1.143	1.076	1.152	1.042	0.888	0.850	0.856	0.814	1.085	1.008	1.041	0.956
	0.1	1.112	0.924	1.034	0.899	1.296	1.159	1.299	1.121	0.962	0.882	0.912	0.851	1.231	1.083	1.187	1.027
	0.3	1.372	1.055	1.302	1.059	1.818	1.511	1.809	1.420	1.189	1.009	1.148	0.996	1.722	1.412	1.659	1.302
	0.5	1.580	1.162	1.514	1.186	2.221	1.821	2.203	1.681	1.354	1.102	1.336	1.105	2.104	1.694	2.018	1.538
	0.7	1.745	1.249	1.658	1.262	2.477	1.968	2.417	1.840	1.475	1.182	1.431	1.169	2.342	1.827	2.194	1.675
	0.9	1.607	1.153	1.503	1.125	2.155	1.736	2.160	1.646	1.296	1.093	1.269	1.044	2.021	1.582	1.905	1.462
	0.95	1.465	1.047	1.351	1.046	1.840	1.527	1.883	1.451	1.161	0.987	1.114	0.971	1.713	1.384	1.616	1.260
	0.99	1.284	0.938	1.189	0.910	1.415	1.253	1.503	1.218	0.986	0.887	0.947	0.850	1.313	1.122	1.247	1.029
Case 2	0.01	6.113	5.767	5.493	5.584	13.469	13.913	12.437	11.866	0.971	0.872	0.930	0.842	1.341	1.111	1.308	1.059
	0.05	6.396	6.031	5.736	5.852	14.105	14.504	13.011	12.401	1.174	0.997	1.123	0.974	1.859	1.447	1.791	1.403
	0.1	6.796	6.364	6.134	6.209	14.937	15.275	13.805	13.090	1.445	1.145	1.366	1.125	2.408	1.783	2.352	1.739
	0.3	8.477	7.815	7.683	7.643	18.446	18.775	17.215	16.165	2.218	1.641	2.193	1.648	3.972	2.952	3.979	2.825
	0.5	10.266	9.464	9.383	9.287	22.463	22.941	21.044	19.733	2.735	2.005	2.753	2.038	5.087	3.825	5.079	3.632
	0.7	12.324	11.479	11.184	11.184	27.115	27.722	25.158	23.957	3.091	2.253	3.023	2.248	5.762	4.204	5.612	4.066
	0.9	14.374	13.696	13.045	13.283	32.072	33.383	29.582	28.545	2.551	1.925	2.547	1.844	4.862	3.486	4.699	3.374
	0.95	14.862	14.268	13.430	13.824	33.289	34.913	30.719	29.807	2.118	1.588	2.081	1.586	3.935	2.836	3.800	2.697
	0.99	15.260	14.748	13.784	14.288	34.285	36.162	31.683	30.780	1.533	1.157	1.494	1.130	2.665	1.903	2.570	1.784
Case 3	0.01	0.977	0.860	0.918	0.826	1.021	1.005	1.041	0.972	0.843	0.825	0.814	0.787	0.969	0.942	0.936	0.889
	0.05	1.030	0.887	0.969	0.857	1.143	1.076	1.152	1.042	0.888	0.850	0.856	0.814	1.085	1.008	1.041	0.956
	0.1	1.112	0.924	1.034	0.899	1.296	1.159	1.299	1.121	0.962	0.882	0.912	0.851	1.231	1.083	1.187	1.027
	0.3	1.372	1.055	1.302	1.059	1.818	1.511	1.809	1.420	1.189	1.009	1.148	0.996	1.722	1.412	1.659	1.302
	0.5	1.580	1.162	1.514	1.186	2.221	1.821	2.203	1.681	1.354	1.102	1.336	1.105	2.104	1.694	2.018	1.538
	0.7	1.745	1.249	1.658	1.262	2.477	1.968	2.417	1.840	1.475	1.182	1.431	1.169	2.342	1.827	2.194	1.675
	0.9	1.607	1.153	1.503	1.125	2.155	1.736	2.160	1.646	1.296	1.093	1.269	1.044	2.021	1.582	1.905	1.462
	0.95	1.465	1.047	1.351	1.046	1.840	1.527	1.883	1.451	1.161	0.987	1.114	0.971	1.713	1.384	1.616	1.260
	0.99	1.284	0.938	1.189	0.910	1.415	1.253	1.503	1.218	0.986	0.887	0.947	0.850	1.313	1.122	1.247	1.029
Case 4	0.01	6.113	5.767	5.493	5.584	13.469	13.913	12.437	11.866	0.971	0.872	0.930	0.842	1.341	1.111	1.308	1.059
	0.05	6.396	6.031	5.736	5.852	14.105	14.504	13.011	12.401	1.174	0.997	1.123	0.974	1.859	1.447	1.791	1.403
	0.1	6.796	6.364	6.134	6.209	14.937	15.275	13.805	13.090	1.445	1.145	1.366	1.125	2.408	1.783	2.352	1.739
	0.3	8.477	7.815	7.683	7.643	18.446	18.775	17.215	16.165	2.218	1.641	2.193	1.648	3.972	2.952	3.979	2.825
	0.5	10.266	9.464	9.383	9.287	22.463	22.941	21.044	19.733	2.735	2.005	2.753	2.038	5.087	3.825	5.079	3.632
	0.7	12.324	11.479	11.184	11.184	27.115	27.722	25.158	23.957	3.091	2.253	3.023	2.248	5.762	4.204	5.612	4.066
	0.9	14.374	13.696	13.045	13.283	32.072	33.383	29.582	28.545	2.551	1.925	2.547	1.844	4.862	3.486	4.699	3.374
	0.95	14.862	14.268	13.430	13.824	33.289	34.913	30.719	29.807	2.118	1.588	2.081	1.586	3.935	2.836	3.800	2.697
	0.99	15.260	14.748	13.784	14.288	34.285	36.162	31.683	30.780	1.533	1.157	1.494	1.130	2.665	1.903	2.570	1.784

Note: The RMSPE is defined as $\sqrt{MSPE_{pre}(\tau)}$ for the proposed method and $\sqrt{MSPE^{\dagger}_{pre}(\tau)}$ for the conventional method.

Table A.2: Simulated averages of the post-intervention biases.

		conventional method								proposed method							
		$T_o = 50$				$T_o = 100$				$T_o = 50$				$T_o = 100$			
		$N = 50$		$N = 100$		$N = 50$		$N = 100$		$N = 50$		$N = 100$		$N = 50$		$N = 100$	
	τ	$n = 50$	100	50	100	50	100	50	100	$n = 50$	100	50	100	50	100	50	100
Case 1	0.01	0.015	0.003	0.010	0.024	0.026	-0.003	0.012	0.006	0.014	-0.007	0.010	0.015	0.023	0.004	0.011	0.002
	0.05	0.022	-0.001	0.007	0.017	0.030	-0.004	0.017	0.011	0.019	-0.010	0.007	0.008	0.028	0.004	0.017	0.010
	0.1	0.032	-0.003	-0.004	0.012	0.029	-0.005	0.015	0.008	0.026	-0.011	-0.004	0.002	0.026	0.003	0.017	0.004
	0.3	0.026	0.008	-0.012	-0.001	0.038	-0.008	-0.009	0.010	0.015	-0.001	-0.006	-0.009	0.029	-0.001	-0.008	0.006
	0.5	0.021	0.009	-0.009	0.005	0.022	-0.004	-0.021	0.000	0.014	0.006	-0.006	0.002	0.016	0.002	-0.023	-0.004
	0.7	0.007	-0.007	0.000	0.000	0.010	-0.017	-0.015	-0.012	0.002	-0.011	0.000	-0.002	0.006	-0.008	-0.013	-0.013
	0.9	-0.011	-0.017	-0.009	-0.008	-0.004	-0.017	-0.021	-0.014	-0.016	-0.020	-0.006	-0.010	-0.008	-0.009	-0.017	-0.014
	0.95	-0.013	-0.017	-0.016	-0.004	-0.011	-0.013	-0.013	-0.017	-0.017	-0.020	-0.013	-0.005	-0.015	-0.006	-0.009	-0.015
	0.99	-0.013	-0.013	-0.020	-0.004	-0.019	-0.018	-0.021	-0.014	-0.015	-0.017	-0.018	-0.006	-0.021	-0.013	-0.015	-0.012
Case 2	0.01	1.710	1.664	1.535	1.609	3.735	3.760	3.449	3.341	0.009	-0.006	0.008	0.011	0.023	0.005	0.006	-0.001
	0.05	1.713	1.656	1.528	1.596	3.722	3.742	3.444	3.352	0.022	-0.012	0.005	-0.002	0.034	0.000	0.020	0.016
	0.1	1.734	1.649	1.512	1.598	3.705	3.748	3.440	3.348	0.040	-0.014	-0.006	-0.014	0.034	0.003	0.016	0.012
	0.3	1.701	1.662	1.497	1.570	3.701	3.736	3.391	3.330	0.019	0.006	-0.016	-0.033	0.065	0.008	-0.025	0.013
	0.5	1.701	1.654	1.511	1.566	3.666	3.717	3.375	3.306	0.023	0.018	-0.012	-0.006	0.033	0.021	-0.040	-0.006
	0.7	1.687	1.644	1.522	1.562	3.638	3.694	3.329	3.273	0.010	-0.011	0.013	-0.009	0.021	-0.010	-0.011	-0.024
	0.9	1.616	1.617	1.488	1.535	3.550	3.673	3.307	3.276	-0.010	-0.023	0.009	-0.019	-0.004	-0.006	-0.018	-0.020
	0.95	1.618	1.613	1.460	1.539	3.546	3.690	3.303	3.255	-0.010	-0.026	-0.005	-0.008	-0.017	0.005	0.002	-0.019
	0.99	1.613	1.609	1.442	1.546	3.530	3.666	3.306	3.260	-0.008	-0.021	-0.010	-0.008	-0.029	-0.016	-0.009	-0.012
Case 3	0.01	0.008	0.007	0.005	0.031	0.027	-0.005	0.009	0.007	0.018	-0.016	0.005	0.019	0.028	-0.014	-0.020	0.000
	0.05	0.019	0.003	0.002	0.022	0.034	-0.006	0.016	0.014	0.011	-0.024	-0.012	-0.002	0.041	-0.038	-0.079	-0.023
	0.1	0.030	0.000	-0.012	0.015	0.032	-0.008	0.016	0.009	0.023	-0.033	-0.054	-0.023	0.055	0.008	-0.134	-0.054
	0.3	0.026	0.013	-0.020	-0.001	0.043	-0.011	-0.012	0.014	-0.005	-0.013	-0.113	-0.065	0.011	0.003	-0.268	-0.185
	0.5	0.022	0.013	-0.017	0.006	0.026	-0.007	-0.026	0.001	-0.023	-0.025	-0.103	-0.065	-0.006	-0.075	-0.295	-0.208
	0.7	0.002	-0.006	-0.007	0.002	0.013	-0.021	-0.021	-0.016	-0.038	-0.048	-0.122	-0.062	-0.025	-0.081	-0.299	-0.213
	0.9	-0.017	-0.018	-0.015	-0.007	-0.004	-0.022	-0.026	-0.019	-0.051	-0.060	-0.112	-0.062	-0.069	-0.045	-0.253	-0.191
	0.95	-0.019	-0.017	-0.022	-0.003	-0.012	-0.018	-0.018	-0.024	-0.027	-0.036	-0.113	-0.039	-0.038	-0.035	-0.265	-0.178
	0.99	-0.020	-0.013	-0.027	-0.002	-0.020	-0.023	-0.027	-0.019	-0.021	-0.021	-0.080	-0.014	-0.010	-0.025	-0.183	-0.086
Case 4	0.01	2.557	2.517	2.351	2.452	5.348	5.324	4.994	4.886	1.424	1.389	1.411	1.413	2.676	2.653	2.621	2.647
	0.05	2.561	2.506	2.341	2.436	5.337	5.299	4.986	4.902	1.411	1.368	1.372	1.383	2.645	2.581	2.485	2.576
	0.1	2.585	2.498	2.321	2.439	5.317	5.307	4.987	4.896	1.425	1.351	1.297	1.352	2.585	2.588	2.365	2.476
	0.3	2.543	2.510	2.303	2.407	5.297	5.287	4.920	4.873	1.330	1.361	1.201	1.259	2.438	2.502	2.133	2.238
	0.5	2.536	2.498	2.324	2.400	5.254	5.268	4.897	4.844	1.278	1.327	1.195	1.239	2.355	2.317	2.113	2.184
	0.7	2.515	2.485	2.333	2.396	5.213	5.230	4.827	4.798	1.262	1.272	1.174	1.215	2.308	2.281	2.088	2.162
	0.9	2.435	2.455	2.286	2.360	5.103	5.199	4.799	4.798	1.248	1.244	1.197	1.226	2.247	2.377	2.091	2.185
	0.95	2.437	2.451	2.252	2.364	5.102	5.218	4.792	4.773	1.270	1.290	1.190	1.267	2.308	2.386	2.134	2.200
	0.99	2.432	2.444	2.231	2.372	5.084	5.197	4.798	4.776	1.320	1.331	1.264	1.347	2.399	2.499	2.204	2.401

Note: The bias is defined as $Bias(\tau) = \frac{1}{T-T_o}\sum_{t=T_o+1}^{T}(\hat{\hat{\delta}}_t(\tau)/\delta_t(\tau) - 1)$ for the proposed method and $Bias^{\dagger}(\tau) = \frac{1}{T-T_o}\sum_{t=T_o+1}^{T}(\hat{\hat{\delta}}_t^{\dagger}(\tau)/\delta_t(\tau) - 1)$ for the conventional method.

Table A.3: Events of the state-level MW hikes in 1990Q1-2006Q2.

		pre-intervention					post-intervention							
		$t \leq T_o$	duration	FMW	SMW	EMW	$t \geq T_o + 1$	duration	FMW	SMW	EMW	ΔFMW	ΔSMW	ΔEMW
California	**1**	**1990Q1-199604**	**28**	**4.75**	**4.25**	**4.75**	**1997Q1-1997Q2**	**2**	**4.75**	**5.00**	**5.00**	**0**	**0.75**	**0.25**
	2	1997Q1-1997Q2	2	4.75	5.00	5.00	1997Q3-1997Q4	2	5.15	5.15	5.15	0.4	0.15	0.15
	3	1997Q3-1997Q4	2	5.15	5.15	5.15	1998Q1-2000Q4	12	5.15	5.75	5.75	0	0.60	0.60
	4	1998Q1-2000Q4	12	5.15	5.75	5.75	2001Q1-2001Q4	4	5.15	6.25	6.25	0	0.50	0.50
	5	2001Q1-2001Q4	4	5.15	6.25	6.25	2002Q1-2006Q2	17	5.15	6.75	6.75	0	0.50	0.50
Florida	1	1990Q1-2005Q1	61	5.15	0.00	5.15	2005Q2-2005Q4	3	5.15	6.15	6.15	0	6.15	1.00
	2	2005Q2-2005Q4	3	5.15	6.15	6.15	2006Q1-2006Q2	1	5.15	6.40	6.40	0	0.25	0.25
Illinois	1	1990Q1-2003Q4	56	5.15	0.00	5.15	2004Q1-2004Q4	4	5.15	5.50	5.50	0	5.50	0.35
	2	2004Q1-2004Q4	4	5.15	5.50	5.50	2005Q1-2006Q2	5	5.15	6.50	6.50	0	1.00	1.00
Iowa	1	1990Q1-1990Q4	4	3.80	3.85	3.85	1991Q1-1991Q4	4	3.80	4.25	4.25	0	0.40	0.40
	2	1991Q1-1991Q4	4	4.25	4.25	4.25	1992Q1-2006Q2	57	4.25	4.65	4.65	0	0.40	0.40
Maryland	1	1990Q1-2005Q4	64	5.15	0.00	5.15	2006Q1-2006Q2	1	5.15	6.15	6.15	0	6.15	1.00
Minnesota	1	1990Q1-1990Q4	4	3.80	3.95	3.95	1991Q1-2005Q2	58	3.80	4.25	4.25	0	0.30	0.30
	2	1991Q1-2005Q2	58	5.15	4.25	5.15	2005Q3-2006Q2	3	5.15	6.15	6.15	0	1.90	1.00
New York	1	1990Q1-2004Q4	60	5.15	0.00	5.15	2005Q1-2005Q4	4	5.15	6.00	6.00	0	6.00	0.85
	2	2005Q1-2005Q4	4	5.15	6.00	6.00	2006Q1-2006Q2	1	5.15	6.75	6.75	0	0.75	0.75
Oregon	1	1990Q1-1990Q4	4	3.80	4.25	4.25	1991Q1-1996Q4	24	3.80	4.75	4.75	0	0.50	0.50
	2	**1991Q1-1996Q4**	**24**	**4.75**	**4.75**	**4.75**	**1997Q1-1997Q4**	**4**	**4.75**	**5.50**	**5.50**	**0**	**0.75**	**0.75**
	3	1997Q1-1997Q4	4	5.15	5.50	5.50	1998Q1-1998Q4	4	5.15	6.00	6.00	0	0.50	0.50
	4	1998Q1-1998Q4	4	5.15	6.00	6.00	1999Q1-2002Q4	16	5.15	6.50	6.50	0	0.50	0.50
	5	1999Q1-2002Q4	16	5.15	6.50	6.50	2003Q1-2005Q4	12	5.15	6.90	6.90	0	0.40	0.40
	6	2003Q1-2005Q4	12	5.15	6.90	6.90	2006Q1-2006Q2	1	5.15	7.50	7.50	0	0.60	0.60
Washington	1	1990Q1-1993Q4	16	4.25	4.25	4.25	1994Q1-1998Q4	20	4.25	4.90	4.90	0	0.65	0.65
	2	**1994Q1-1998Q4**	**20**	**5.15**	**4.90**	**5.15**	**1999Q1-1999Q4**	**4**	**5.15**	**5.70**	**5.70**	**0**	**0.80**	**0.55**
	3	1999Q1-1999Q4	4	5.15	5.70	5.70	2000Q1-2000Q4	4	5.15	6.50	6.50	0	0.80	0.80
	4	2000Q1-2000Q4	4	5.15	6.50	6.50	2001Q1-2001Q4	4	5.15	6.72	6.72	0	0.22	0.22
	5	2001Q1-2001Q4	4	5.15	6.72	6.72	2002Q1-2002Q4	4	5.15	6.90	6.90	0	0.18	0.18
	6	2002Q1-2002Q4	4	5.15	6.90	6.90	2003Q1-2003Q4	4	5.15	7.01	7.01	0	0.11	0.11
	7	2003Q1-2003Q4	4	5.15	7.01	7.01	2004Q1-2004Q4	4	5.15	7.16	7.16	0	0.15	0.15
	8	2004Q1-2004Q4	4	5.15	7.16	7.16	2005Q1-2005Q4	4	5.15	7.35	7.35	0	0.19	0.19
	9	2005Q1-2005Q4	4	5.15	7.35	7.35	2006Q1-2006Q2	1	5.15	7.63	7.63	0	0.28	0.28
Wisconsin	**1**	**1990Q1-2005Q1**	**61**	**5.15**	**3.65**	**5.15**	**2005Q2-2006Q2**	**5**	**5.15**	**5.70**	**5.70**	**0**	**2.05**	**0.55**

Note: The entries in boldface correspond to the four case studies. In Case 1, the treated state is California, and the intervention occurred in 1997Q1. In Case 2, the treated state is Oregon, and the intervention occurred in 1997Q1. In Case 3, the treated state is Washington, and the intervention occurred in 1999Q1. In Case 4, the treated state is Wisconsin, and the intervention occurred in 2005Q2. "FMW" means the federal MW, "SMW" means the state-level MW, and "EMW" means the effective MW (that is, EMW=max(FMW,EMW)). In addition, $\Delta\text{FMW} := \text{FMW}_t - \text{FMW}_{T_o}$, for $t \geq T_o + 1$; ΔSMW and ΔEMW are similarly defined.

Table A.4: Economic features of the treated state and its synthetic-control states.

	Case 1: California (labor earnings)						Case 2: Oregon (labor earnings)					
	treated	synth(avg)	synth(10%)	synth(50%)	synth(90%)	single	treated	synth(avg)	synth(10%)	synth(50%)	synth(90%)	single
pop	30996.98	17190.74	17260.76	17262.56	17157.34	3556.71	3029.91	3804.65	3704.08	3656.14	4082.64	3556.71
white	80.40	85.60	85.61	85.61	85.67	92.68	93.96	90.79	90.17	91.31	89.88	92.68
old	10.45	10.92	10.93	10.93	11.01	10.45	13.85	13.00	12.86	13.00	13.04	10.45
land	155779.22	232806.94	234326.59	234391.39	230602.97	103641.89	95988.01	86153.70	84736.31	88741.23	81858.31	103641.89
pov	17.17	16.99	17.00	17.01	16.91	9.47	11.60	11.80	11.81	11.78	11.89	9.47
pinc	23376.41	20076.26	20100.40	20099.68	20141.48	22918.66	20800.11	20951.48	20818.15	21112.79	20552.01	22918.66
hinc	59264.43	50406.68	50372.40	50371.45	50426.82	59468.86	54598.14	52445.41	52478.37	52574.87	52150.70	59468.86
emp	12392.73	7287.75	7308.91	7309.64	7267.00	1689.17	1341.29	1725.55	1682.98	1656.46	1851.13	1689.17
nhu	8647.29	6198.83	6205.06	6206.10	6145.20	2325.71	1821.29	1650.78	1736.47	1570.98	1844.57	2325.71

	Case 3: Washington (labor earnings)						Case 4: Wisconsin (labor earnings)					
	treated	synth(avg)	synth(10%)	synth(50%)	synth(90%)	single	treated	synth(avg)	synth(10%)	synth(50%)	synth(90%)	single
pop	5318.75	5610.95	5474.28	5590.42	5588.44	5281.55	5214.22	6021.63	5634.26	5965.24	5628.03	9747.59
white	89.48	86.21	84.88	86.36	85.68	87.46	91.55	86.49	87.25	86.44	86.68	82.83
old	11.34	11.65	11.49	11.42	11.39	13.79	12.77	12.96	12.78	13.02	12.62	12.51
land	66455.52	66420.61	67596.71	70227.39	66822.11	68741.52	54157.80	51661.24	47357.35	52189.91	50802.48	56538.90
pov	10.84	10.29	10.50	10.38	10.24	11.22	9.11	10.28	10.19	10.13	10.20	11.29
pinc	24243.51	23408.68	23722.59	23534.28	23715.10	21653.12	25802.82	25147.72	24718.27	25125.56	24888.57	25863.98
hinc	60570.00	59165.88	59666.06	59268.53	59895.17	52144.89	60591.33	55886.80	54944.91	56076.65	55232.31	59331.80
emp	2343.54	2630.01	2554.92	2597.73	2645.85	2474.00	2610.89	2827.74	2663.79	2818.98	2664.20	4304.83
nhu	3463.78	3124.46	3195.33	3171.48	2991.18	1851.89	2744.00	2693.56	2687.58	2653.63	2843.14	3916.40

	Case 1: California (employment)						Case 2: Oregon (employment)					
	treated	synth(avg)	synth(10%)	synth(50%)	synth(90%)	single	treated	synth(avg)	synth(10%)	synth(50%)	synth(90%)	single
pop	30996.98	17234.02	17272.58	17272.80	17256.79	3556.71	3029.91	3636.03	3603.58	3594.39	3785.90	3556.71
white	80.40	85.61	85.60	85.60	85.58	92.68	93.96	91.45	91.58	91.59	91.52	92.68
old	10.45	10.93	10.92	10.92	10.90	10.45	13.85	13.04	13.04	13.03	13.22	10.45
land	155779.22	233722.74	234752.30	234760.05	234717.89	103641.89	95988.01	89719.97	90230.66	90375.59	87107.13	103641.89
pov	17.17	17.00	17.02	17.02	17.03	9.47	11.60	11.81	11.80	11.79	11.82	9.47
pinc	23376.41	20095.38	20095.70	20095.61	20074.25	22918.66	20800.11	21161.50	21193.85	21206.98	21026.44	22918.66
hinc	59264.43	50387.90	50366.18	50366.06	50373.12	59468.86	54598.14	52511.68	52533.91	52569.17	52310.05	59468.86
emp	12392.73	7299.96	7313.70	7313.79	7311.15	1689.17	1341.29	1644.51	1629.85	1626.03	1713.47	1689.17
nhu	8647.29	6200.53	6211.90	6212.03	6221.06	2325.71	1821.29	1531.25	1510.96	1512.18	1540.98	2325.71

	Case 3: Washington (employment)						Case 4: Wisconsin (employment)					
	treated	synth(avg)	synth(10%)	synth(50%)	synth(90%)	single	treated	synth(avg)	synth(10%)	synth(50%)	synth(90%)	single
pop	5318.75	5395.26	5381.90	5379.27	5528.16	5281.55	5214.22	5496.50	5358.74	5385.22	5768.47	9747.59
white	89.48	85.70	85.69	85.65	86.02	87.46	91.55	87.08	86.49	86.71	87.64	82.83
old	11.34	11.37	11.31	11.33	11.58	13.79	12.77	12.70	12.61	12.62	12.85	12.51
land	66455.52	69201.86	70165.61	69802.22	65680.99	68741.52	54157.80	49648.72	49476.12	48883.85	47935.29	56538.90
pov	10.84	10.45	10.49	10.48	10.22	11.22	9.11	9.99	9.95	9.93	9.94	11.29
pinc	24243.51	23712.62	23792.73	23783.32	23485.76	21653.12	25802.82	25434.54	25636.48	25562.97	25368.07	25863.98
hinc	60570.00	59673.96	59768.84	59773.54	59692.38	52144.89	60591.33	56418.05	56941.53	56762.65	56020.47	59331.80
emp	2343.54	2542.87	2537.03	2536.73	2598.68	2474.00	2610.89	2590.30	2534.98	2547.50	2709.28	4304.83
nhu	3463.78	3175.19	3178.30	3179.83	3164.91	1851.89	2744.00	2691.89	2710.54	2716.76	2707.86	3916.40

Note: "treated" means the treated state, "synth(avg)" means the average synthetic-control state, "synth(10%)" means the 0.1-quantile synthetic-control state, "synth(50%)" means the 0.5-quantile synthetic-control state, "synth(90%)" means the 0.9-quantile synthetic-control state, and "single" means the single-best control state which is Colorado for Case 1 (California) and Case 2 (Oregon), Missouri for Case 3 (Washington), or Michigan for Case 4 (Wisconsin). The economic features of the treated state are invariant to the outcome variables (labor earnings or employment) by construction. In comparison, the synthetic-control state and hence its economic features could change with the outcome variable being considered.

Table A.5: Mean and quantile effects of the state-level MW hikes.

		mean			$\tau = 0.1$			$\tau = 0.5$			$\tau = 0.9$		
(A) labor earnings		$\hat{\mu}_t$	$\hat{\hat{\delta}}_{\mu,t}$	$e_{\mu,t}$	$\hat{q}_t(\tau)$	$\hat{\hat{\delta}}_t(\tau)$	$e_t(\tau)$	$\hat{q}_t(\tau)$	$\hat{\hat{\delta}}_t(\tau)$	$e_t(\tau)$	$\hat{q}_t(\tau)$	$\hat{\hat{\delta}}_t(\tau)$	$e_t(\tau)$
	1996Q4	173.244			140.301			167.322			225.716		
Case 1	1997Q1	169.847	-0.919	-0.102	140.729	-0.419	-0.056	164.833	0.023	0.003	212.474	-5.595	-0.487
(California)	1997Q2	177.098	-0.748	-0.080	146.845	1.138	0.148	172.365	-1.522	-0.166	226.564	1.932	0.163
(ΔEMW=0.25)	p-value		0.857			0.905			0.905			0.476	
	1996Q4	165.240			147.517			166.294			193.845		
Case 2	1997Q1	162.901	3.475	0.138	147.497	6.684	0.301	161.406	2.420	0.096	188.567	1.356	0.046
(Oregon)	1997Q2	172.426	6.787	0.259	157.921	9.230	0.393	168.177	1.083	0.041	204.251	7.377	0.237
(ΔEMW=0.75)	1997Q3	180.003	2.138	0.076	162.537	1.409	0.055	179.468	1.521	0.054	214.981	3.723	0.112
	1997Q4	177.718	4.734	0.173	155.278	0.376	0.015	174.683	-0.661	-0.024	213.285	8.486	0.262
	p-value		0.048			0.143			0.905			0.333	
	1998Q4	177.346			155.488			174.784			195.780		
Case 3	1999Q1	167.683	4.151	0.238	145.650	2.659	0.174	167.456	3.245	0.185	187.162	5.317	0.274
(Washington)	1999Q2	180.284	7.664	0.416	160.661	6.923	0.422	183.203	8.746	0.469	202.254	13.726	0.682
(ΔEMW=0.55)	1999Q3	190.345	7.867	0.404	166.059	4.280	0.248	194.460	13.106	0.677	208.351	9.436	0.444
	1999Q4	187.207	7.583	0.395	164.546	3.157	0.183	190.297	9.736	0.505	211.435	13.899	0.659
	p-value		0.043			0.696			0.043			0.000	
	2004Q4	152.660			125.242			154.478			183.379		
Case 4	2005Q2	160.738	-3.133	-0.179	128.759	-4.386	-0.308	161.331	-5.261	-0.296	188.745	-4.449	-0.216
(Wisconsin)	2005Q3	179.611	-0.354	-0.018	149.151	1.150	0.073	179.169	-0.120	-0.006	214.938	2.332	0.103
(ΔEMW=0.55)	2005Q4	167.504	-3.264	-0.179	143.998	3.881	0.259	165.542	-4.959	-0.272	200.475	-2.068	-0.096
	2006Q1	164.885	0.863	0.049	136.048	3.480	0.246	166.005	2.517	0.144	193.087	-1.329	-0.064
	2006Q2	167.641	-1.053	-0.058	142.522	1.215	0.081	168.447	-0.908	-0.050	197.252	-1.392	-0.066
	p-value		0.353			0.647			0.412			0.882	
(B) employment													
	1996Q4	12636			633			5179			31115		
Case 1	1997Q1	12478	9	0.013	629	65	2.202	4956	-134	-0.500	30574	-549	-0.335
(California)	1997Q2	12937	-108	-0.157	676	63	1.961	5086	-255	-0.908	32176	-354	-0.207
(ΔEMW=0.25)	p-value		0.619			0.190			0.048			0.714	
	1996Q4	2924			540			1664			8011		
Case 2	1997Q1	2815	-56	-0.124	495	-14	-0.178	1488	-37	-0.155	7806	-255	-0.200
(Oregon)	1997Q2	2969	-76	-0.159	578	0	-0.005	1744	114	0.445	8138	-312	-0.234
(ΔEMW=0.75)	1997Q3	3031	-144	-0.287	631	-4	-0.039	1745	41	0.151	8278	-380	-0.278
	1997Q4	2931	-156	-0.320	578	6	0.063	1587	14	0.055	8110	-489	-0.360
	p-value		0.048			0.952			0.429			0.333	
	1998Q4	3702			449			1303			10984		
Case 3	1999Q1	3687	-70	-0.175	426	31	0.745	1280	-40	-0.287	10985	-204	-0.170
(Washington)	1999Q2	3795	-85	-0.205	478	35	0.744	1374	5	0.034	11056	-444	-0.362
(ΔEMW=0.55)	1999Q3	3848	-142	-0.333	493	36	0.735	1460	8	0.050	11308	-287	-0.232
	1999Q4	3803	-230	-0.534	398	-34	-0.734	1370	-121	-0.760	11503	-174	-0.140
	p-value		0.087			0.609			0.522			0.652	
	2004Q4	2128			324			1018			4331		
Case 4	2005Q2	2223	-34	-0.141	305	-30	-0.836	1139	5	0.041	4533	12	0.026
(Wisconsin)	2005Q3	2239	-71	-0.287	322	-35	-0.917	1187	37	0.299	4577	193	0.413
(ΔEMW=0.55)	2005Q4	2157	-149	-0.606	297	-49	-1.321	1082	-58	-0.477	4447	-240	-0.479
	2006Q1	2220	-3	-0.014	360	38	1.116	1088	7	0.060	4381	-177	-0.364
	2006Q2	2231	-84	-0.341	307	-29	-0.818	1135	-50	-0.392	4552	34	0.071
	p-value		0.471			0.471			0.706			0.941	

Note: $\Delta EMW := EMW_{T_o+1} - EMW_{T_o}$. $\hat{\mu}_t$ stands for sample mean, $\hat{\hat{\delta}}_{\mu,t}$ is the estimated mean effect, $e_{\mu,t}$ is the elasticity of the mean effect, $\hat{q}_t(\tau)$ stands for the sample τ-quantile, $\hat{\hat{\delta}}_t(\tau)$ is the estimated τ-quantile effect and $e_t(\tau)$ is the elasticity of the quantile effect. "p-value" means the p-value of the placebo test introduced in Section 3. Recall that $\hat{q}_t(\tau)$ is computed using the default setting of R, and $\hat{\mu}_t$ is a numerical integration of $\hat{q}_t(\cdot)$.

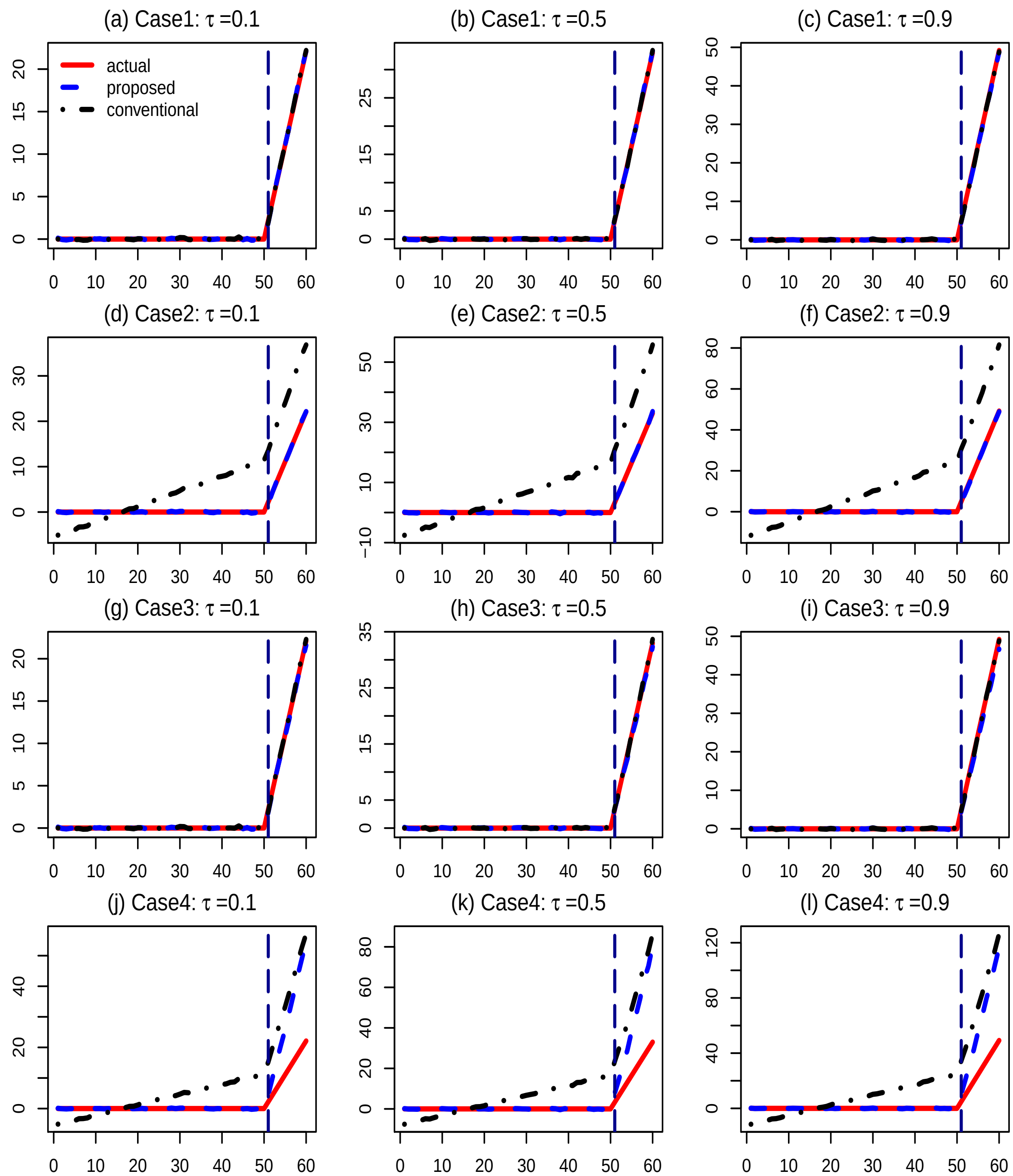


Figure A. 1: The true intervention effects $\{\delta_t(\tau)\}_{t=T_o+1}^{T}$ (red solid lines) and the estimates generated by the proposed method (blue dashed lines) and by the conventional method (black dashed-dotted lines) in the case where $(T_o, N, n) = (50, 50, 100)$. The vertical dashed lines are evaluated at $t = T_o + 1$.

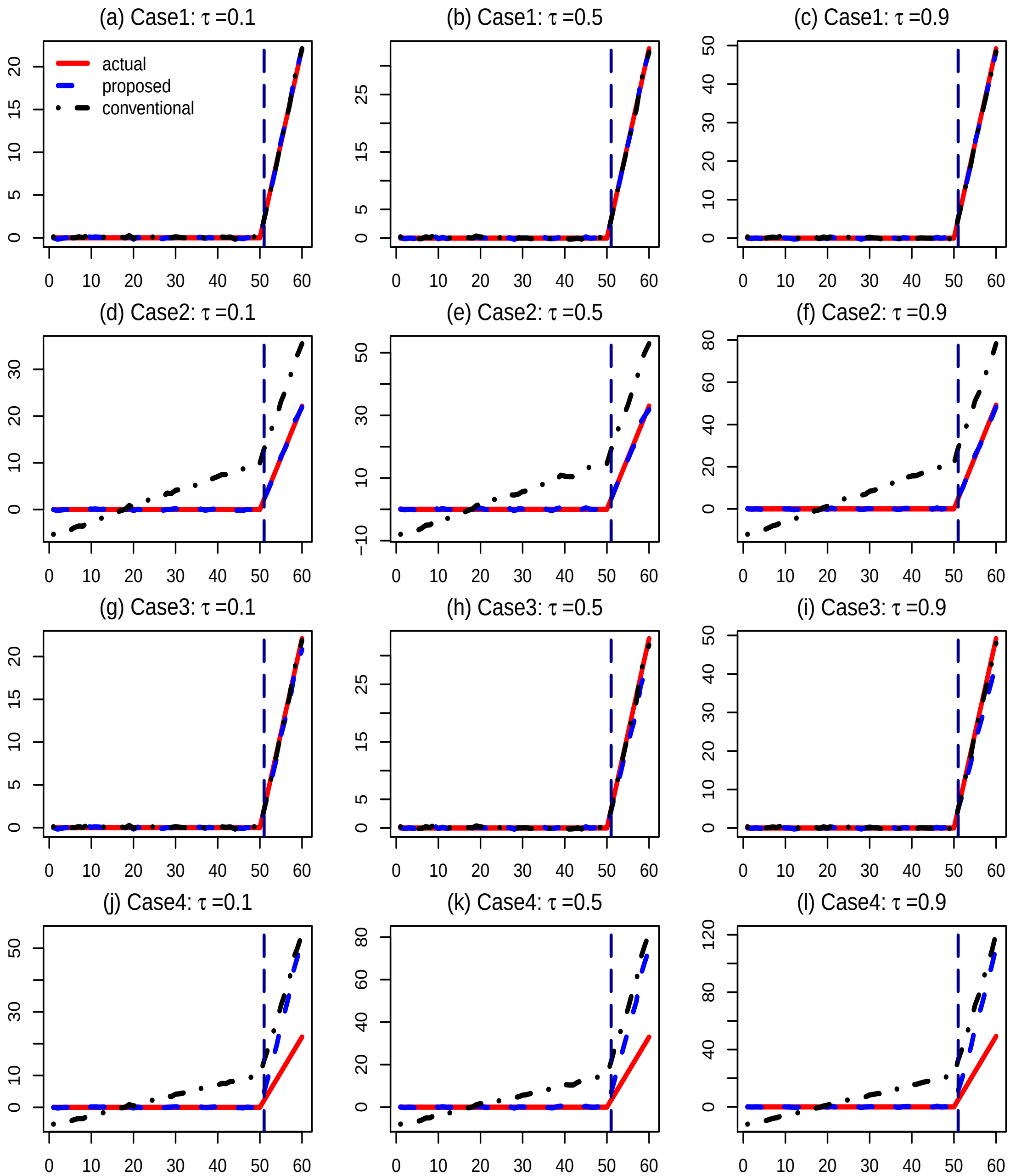


Figure A.2: The true intervention effects $\{\delta_t(\tau)\}_{t=T_o+1}^{T}$ (red solid lines) and the estimates generated by the proposed method (blue dashed lines) and by the conventional method (black dashed-dotted lines) in the case where $(T_o, N, n) = (50, 100, 50)$. The vertical dashed lines are evaluated at $t = T_o + 1$.

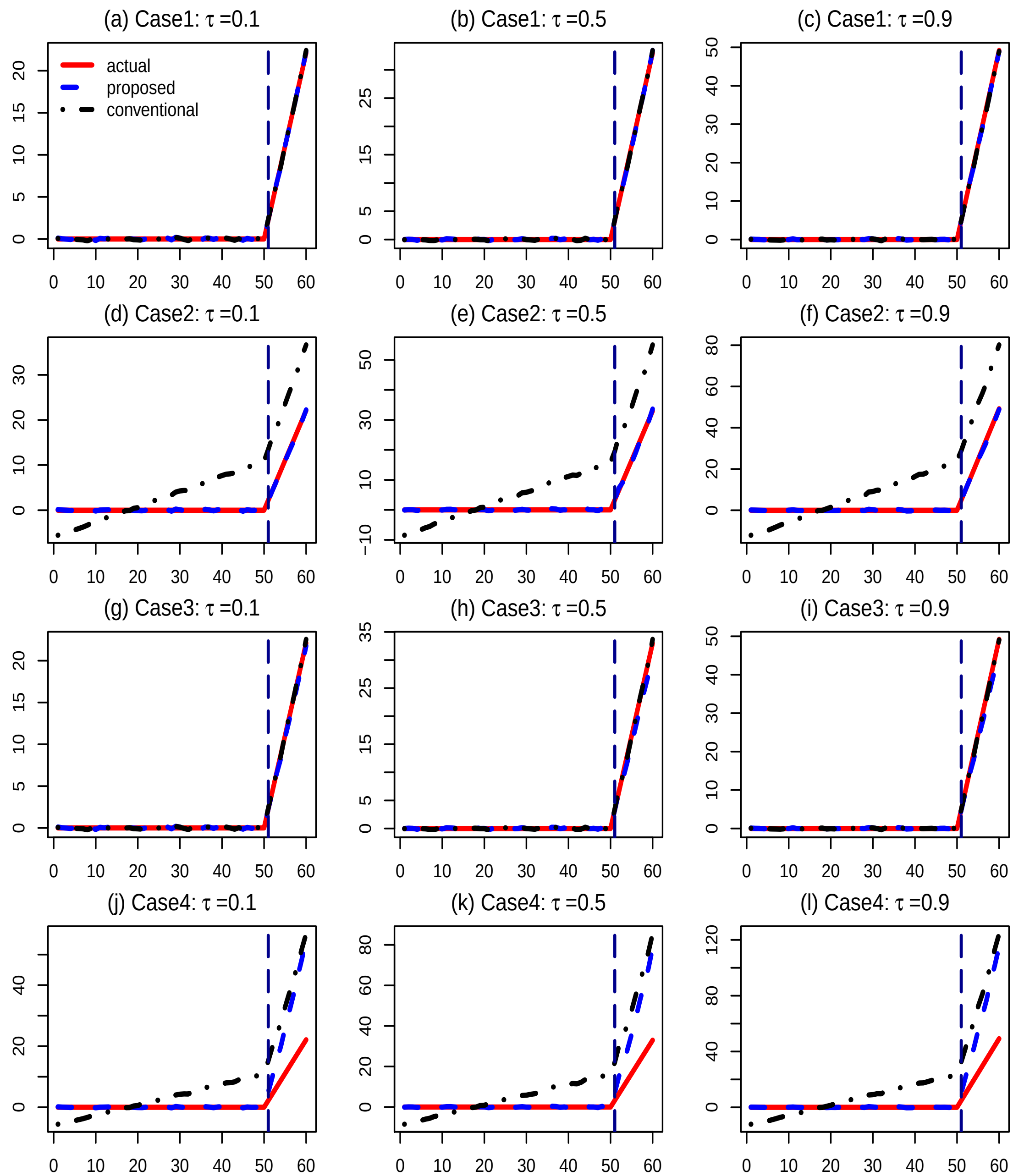


Figure A.3: The true intervention effects $\{\delta_t(\tau)\}_{t=T_o+1}^{T}$ (red solid lines) and the estimates generated by the proposed method (blue dashed lines) and by the conventional method (black dashed-dotted lines) in the case where $(T_o, N, n) = (50, 100, 100)$. The vertical dashed lines are evaluated at $t = T_o + 1$.

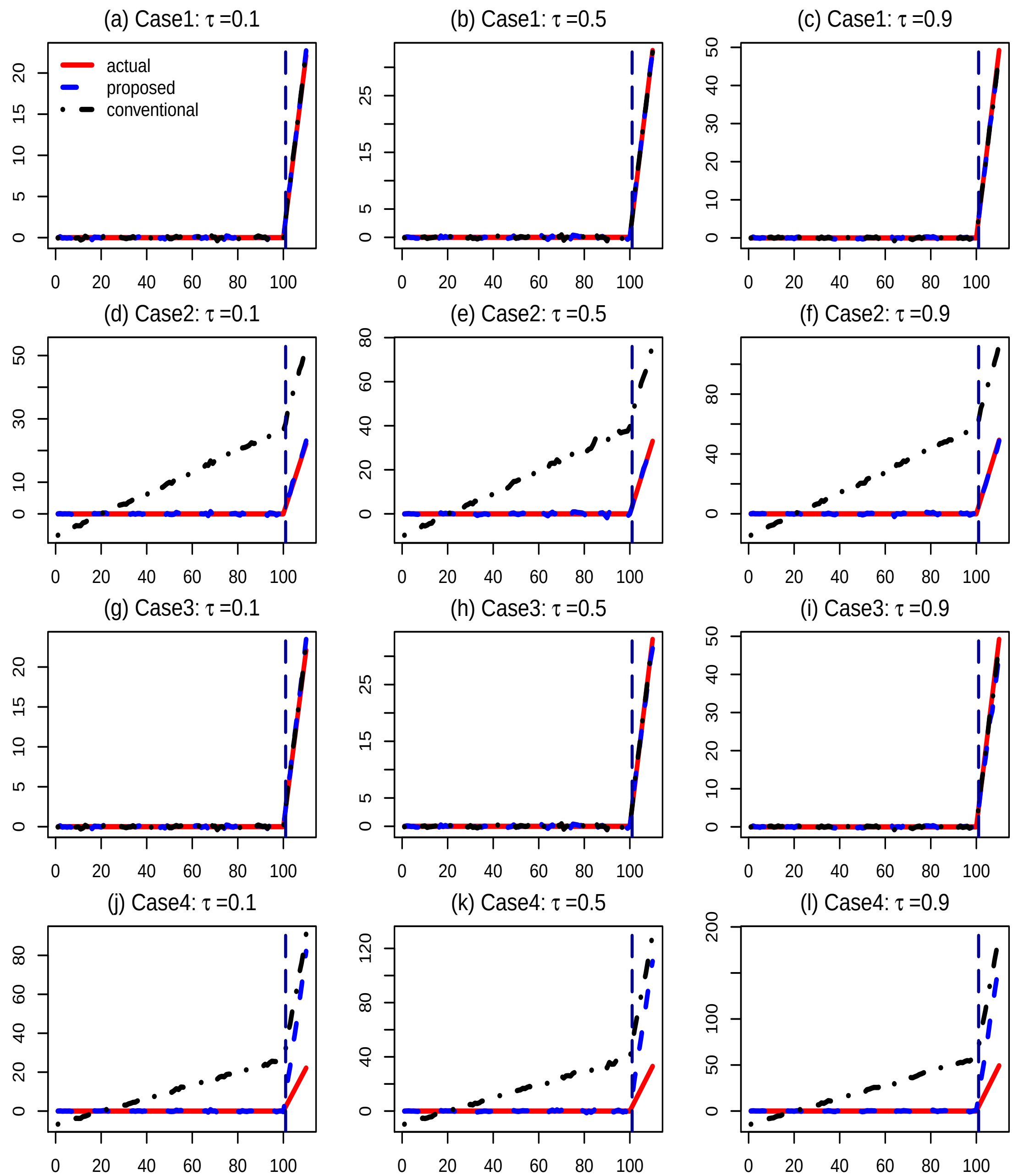


Figure A.4: The true intervention effects $\{\delta_t(\tau)\}_{t=T_o+1}^{T}$ (red solid lines) and the estimates generated by the proposed method (blue dashed lines) and by the conventional method (black dashed-dotted lines) in the case where $(T_o, N, n) = (100, 50, 50)$. The vertical dashed lines are evaluated at $t = T_o + 1$.

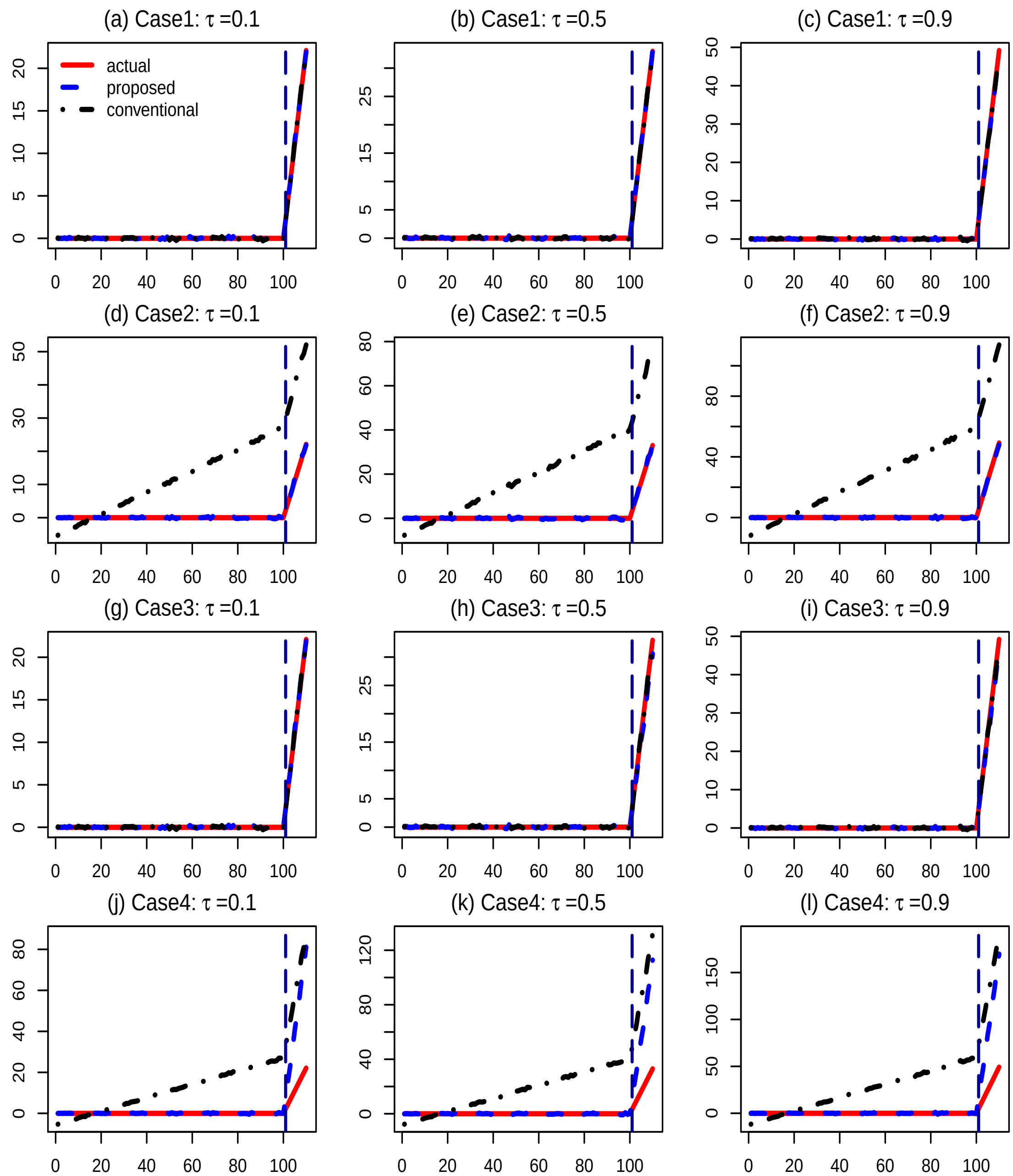


Figure A.5: The true intervention effects $\{\delta_t(\tau)\}_{t=T_o+1}^{T}$ (red solid lines) and the estimates generated by the proposed method (blue dashed lines) and by the conventional method (black dashed-dotted lines) in the case where $(T_o, N, n) = (100, 50, 100)$. The vertical dashed lines are evaluated at $t = T_o + 1$.

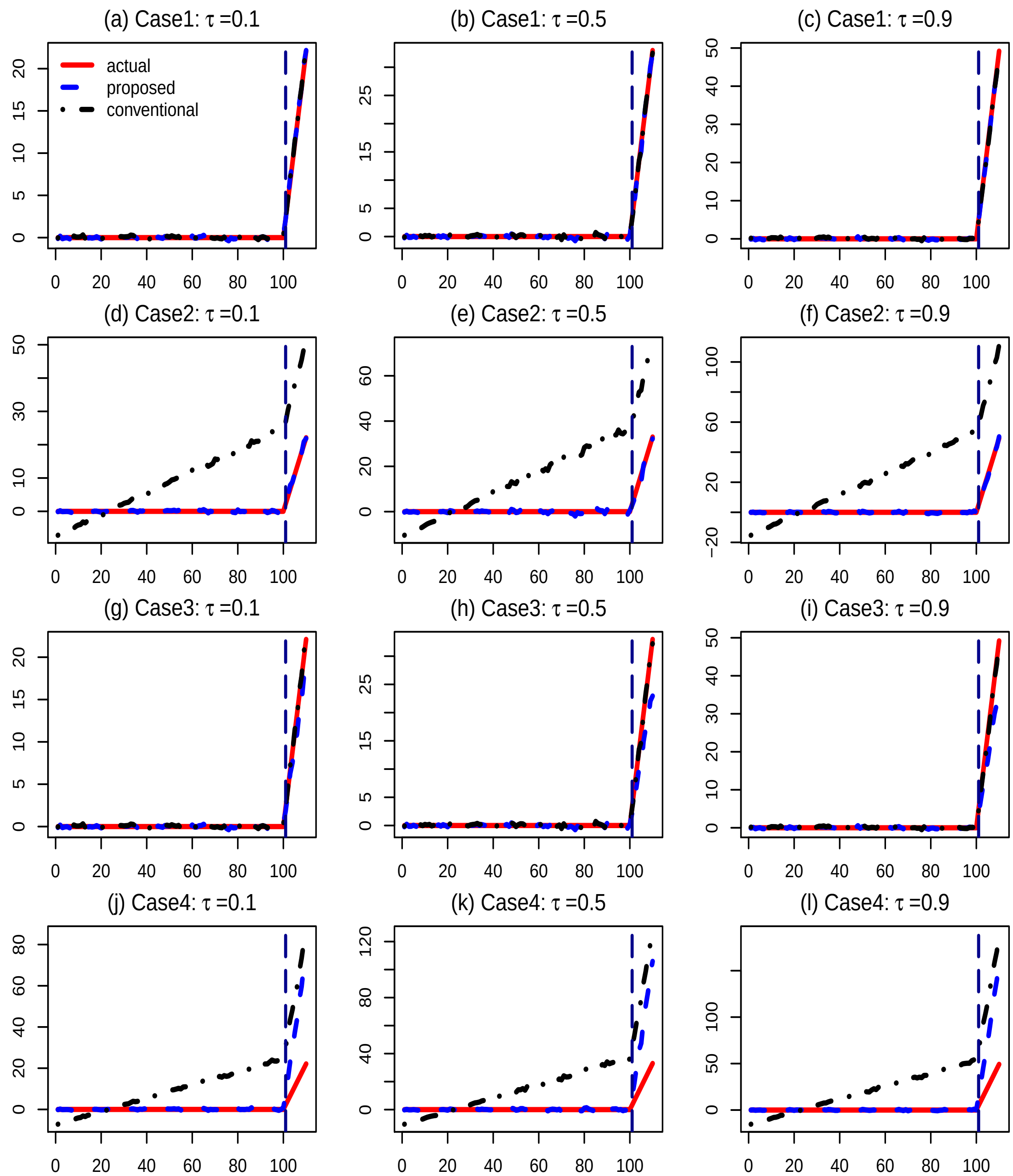


Figure A.6: The true intervention effects $\{\delta_t(\tau)\}_{t=T_o+1}^{T}$ (red solid lines) and the estimates generated by the proposed method (blue dashed lines) and by the conventional method (black dashed-dotted lines) in the case where $(T_o, N, n) = (100, 100, 50)$. The vertical dashed lines are evaluated at $t = T_o + 1$.

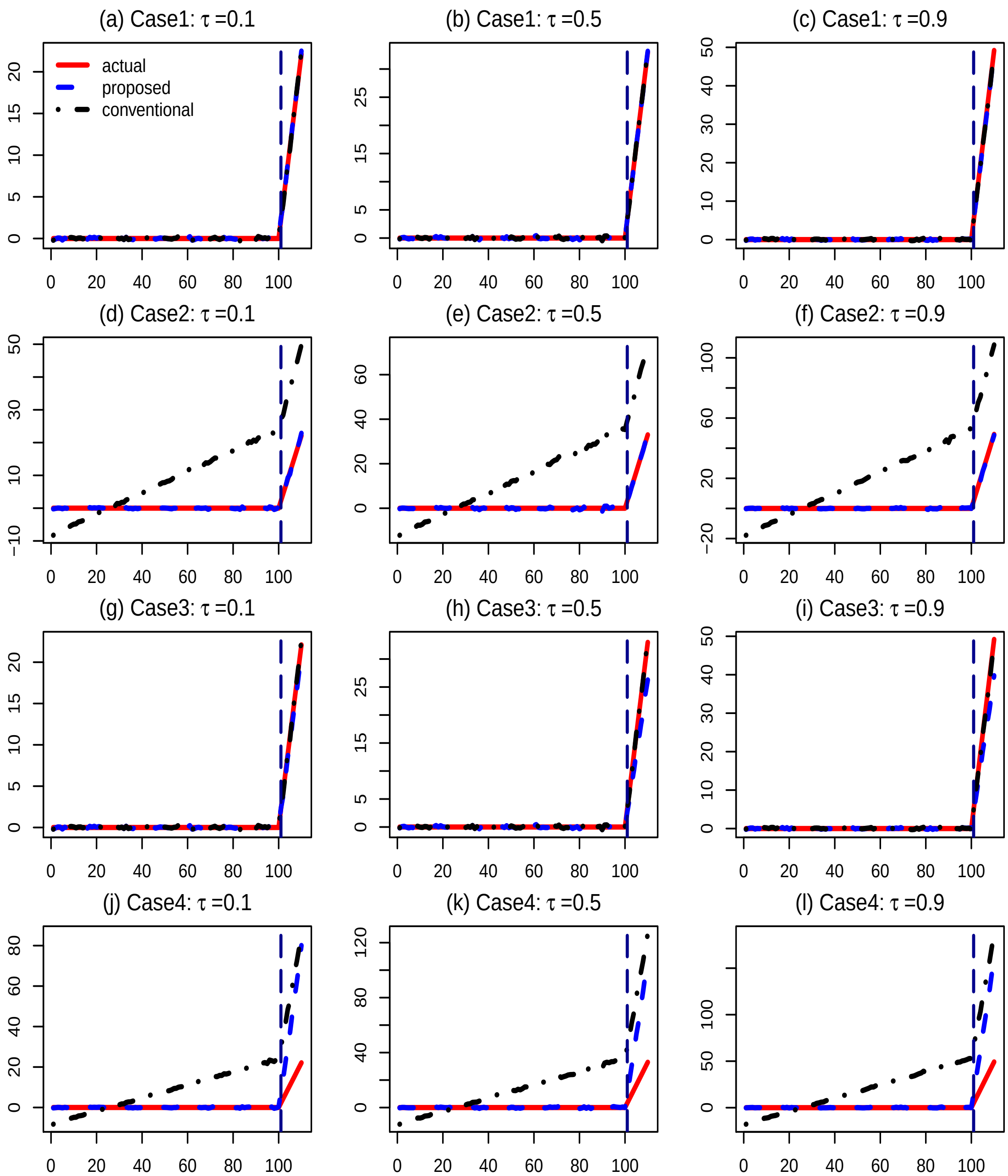


Figure A.7: The true intervention effects $\{\delta_t(\tau)\}_{t=T_o+1}^{T}$ (red solid lines) and the estimates generated by the proposed method (blue dashed lines) and by the conventional method (black dashed-dotted lines) in the case where $(T_o, N, n) = (100, 100, 100)$. The vertical dashed lines are evaluated at $t = T_o + 1$.

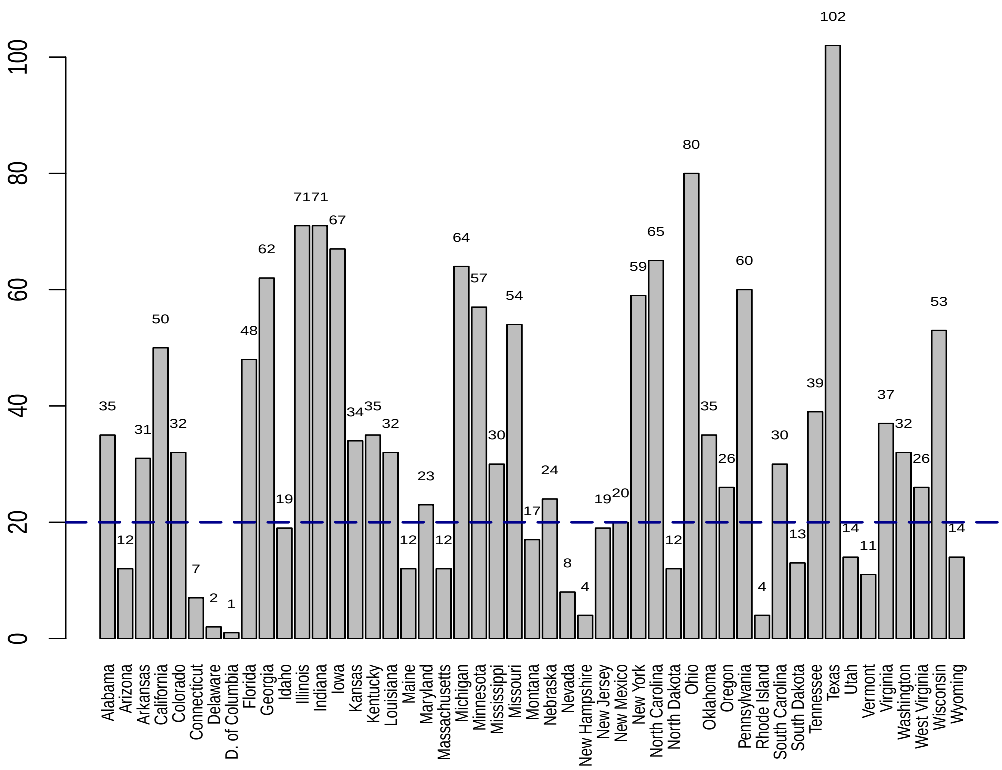


Figure A.8: Number of counties with complete observations for each state.

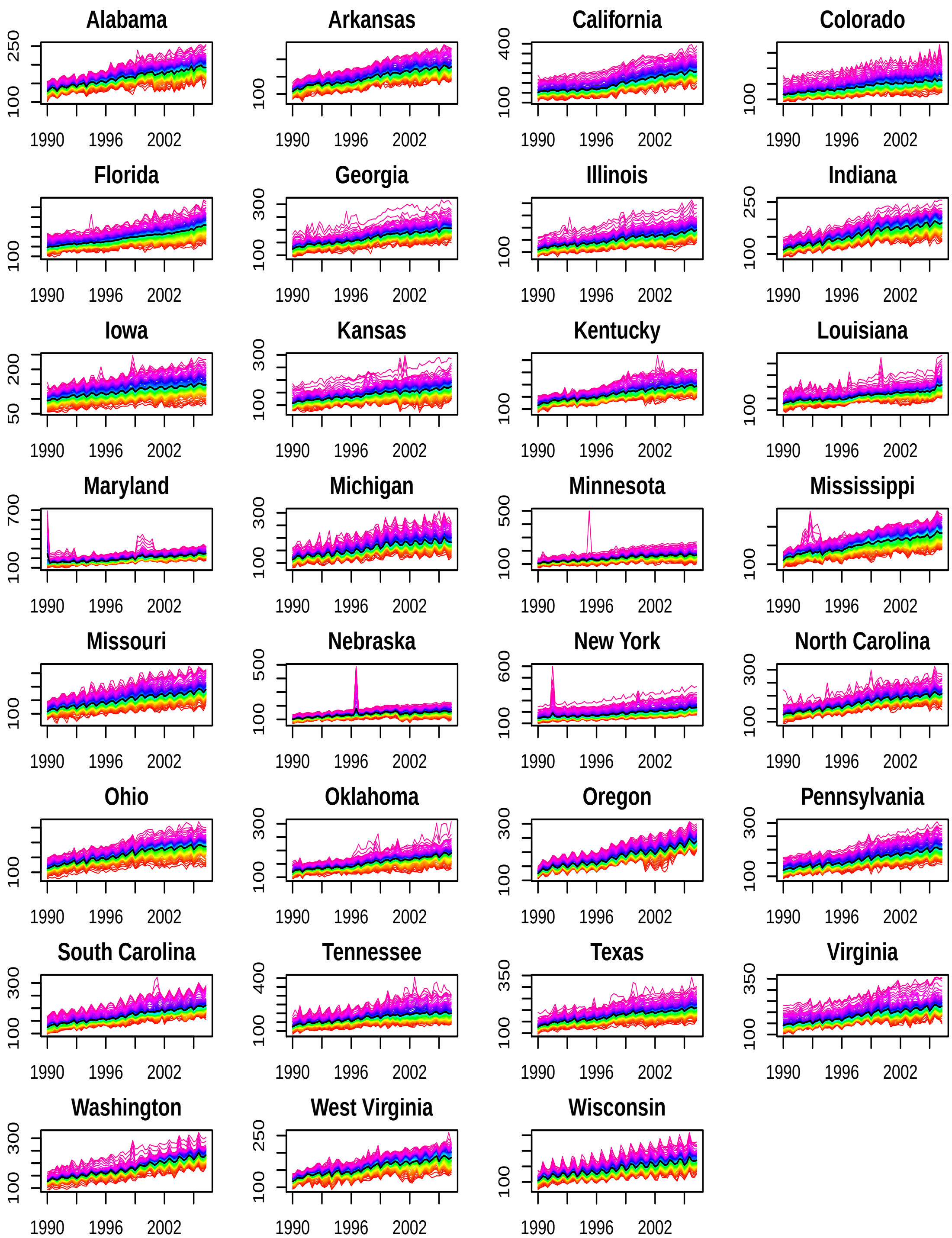


Figure A.9: The county-level mean and quantile sequences of the average weekly earnings of restaurant workers for the states satisfying the data requirement: $n_{it} > 20$. The mean sequence is in black, and the quantile sequences are in the order of a rainbow ranging from red ($\tau = 0.01$) to violet ($\tau = 0.99$).

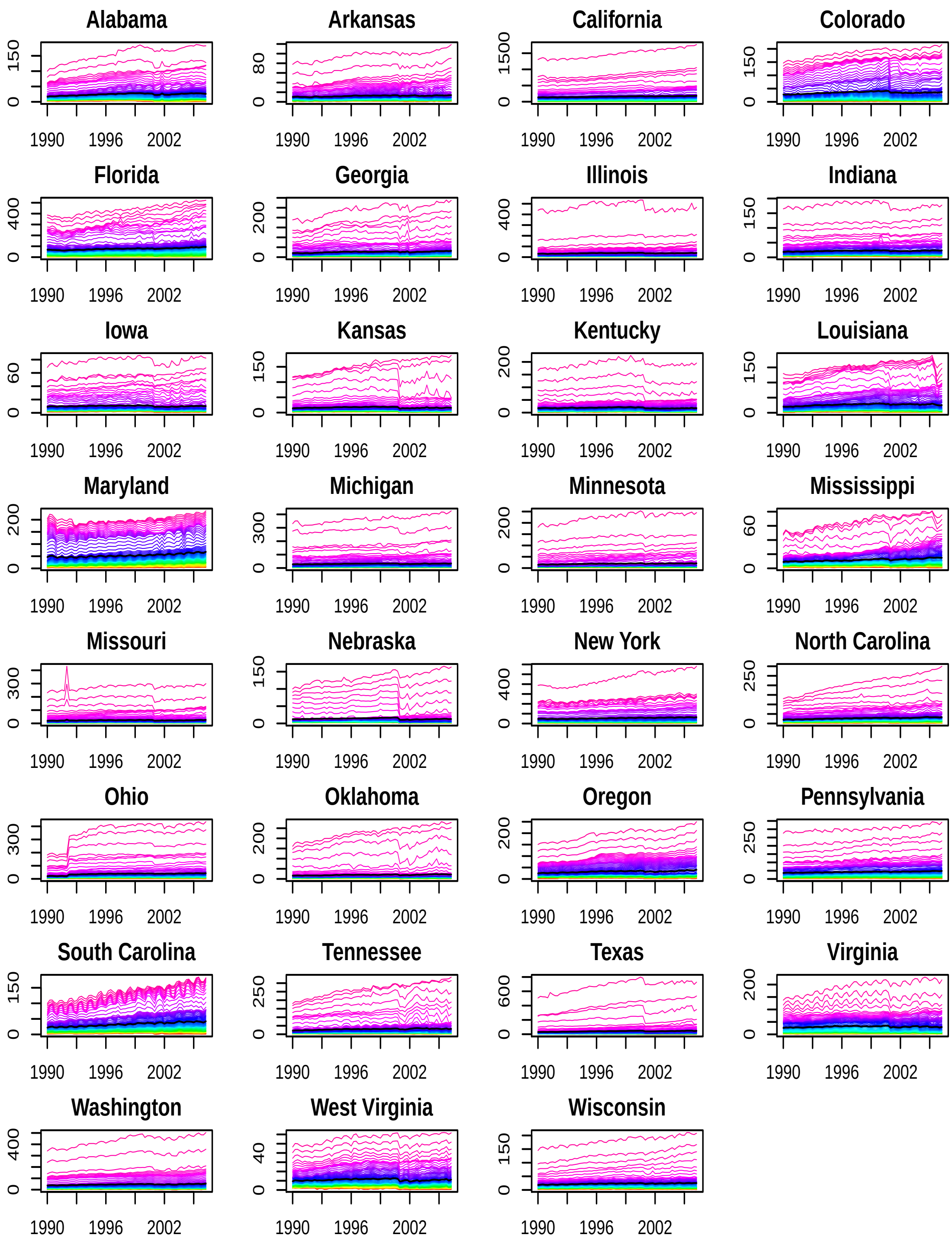


Figure A.10: The county-level mean and quantile sequences of the employment (scaled by 1/100) of restaurant workers for the states satisfying the data requirement: $n_{it} > 20$. The mean sequence is in black, and the quantile sequences are in the order of a rainbow ranging from red ($\tau = 0.01$) to violet ($\tau = 0.99$).

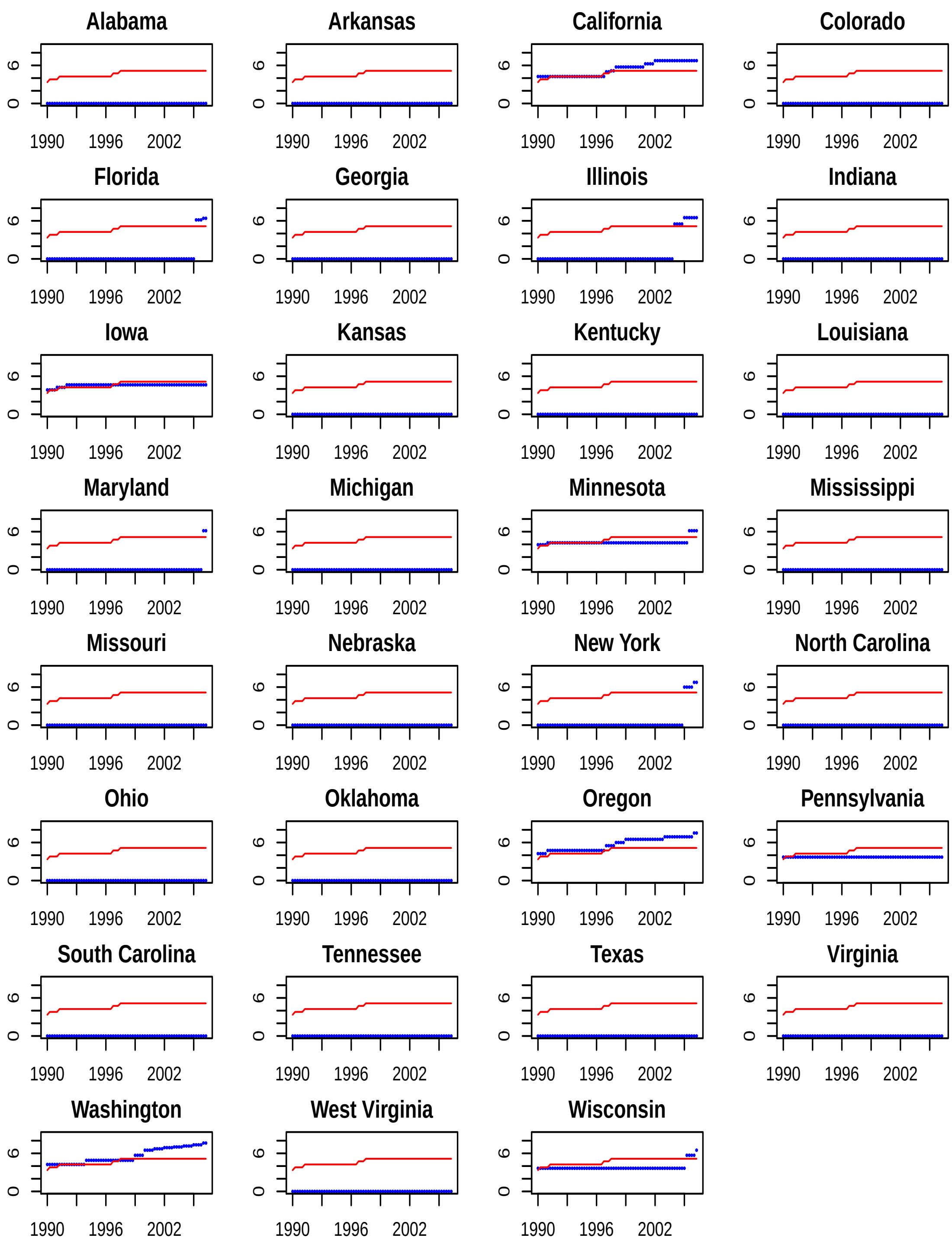


Figure A.11: Time sequences of Federal MW (red lines) and state-level MW (blue dots).

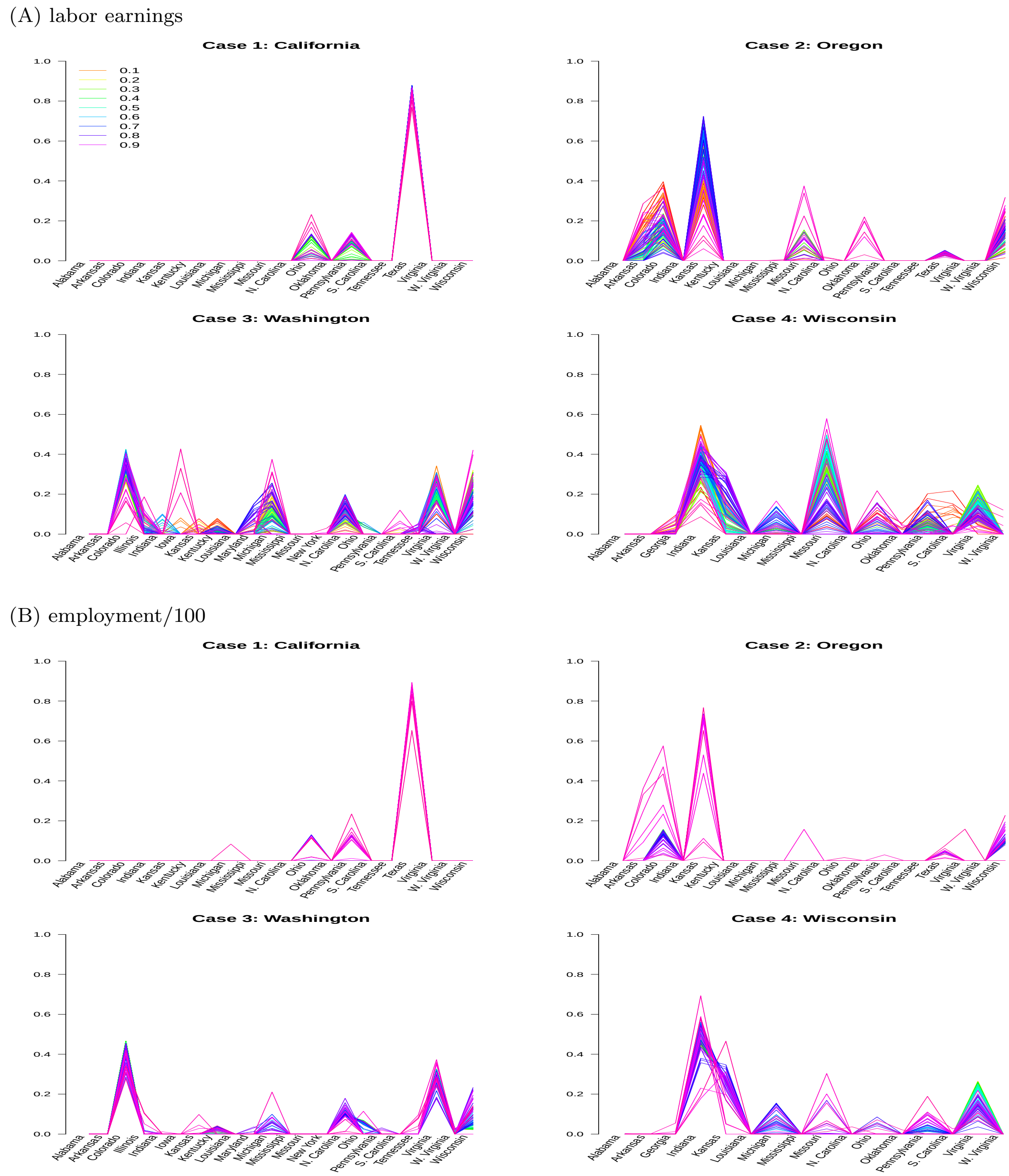
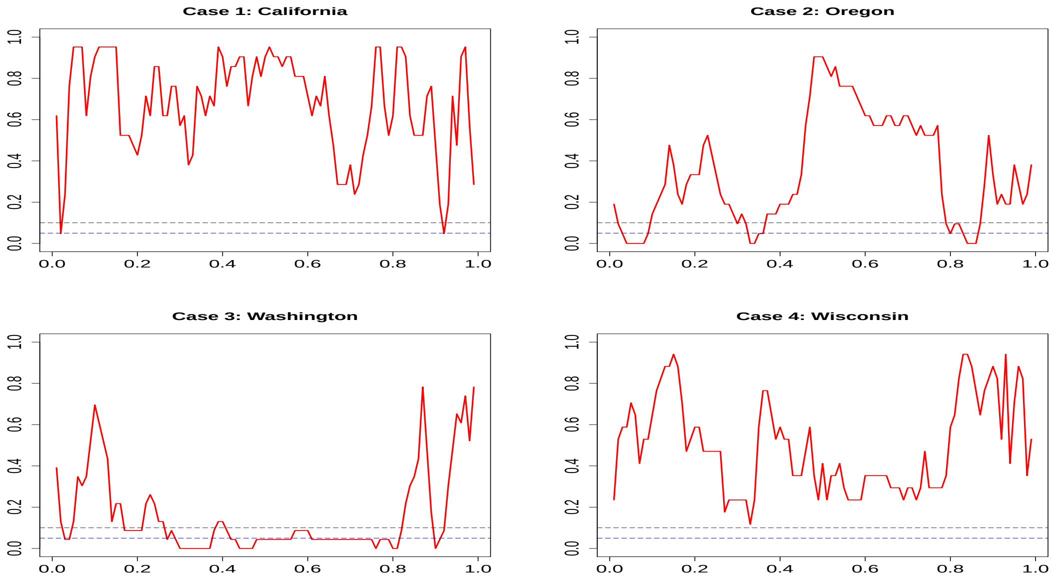


Figure A. 12: Potential control states and the estimates of the τ-quantile synthetic control weight $\boldsymbol{w}^o(\tau)$. The estimated weights are in the order of a rainbow ranging from red ($\tau = 0.01$) to violet ($\tau = 0.99$).

(A) labor earnings



(B) employment/100

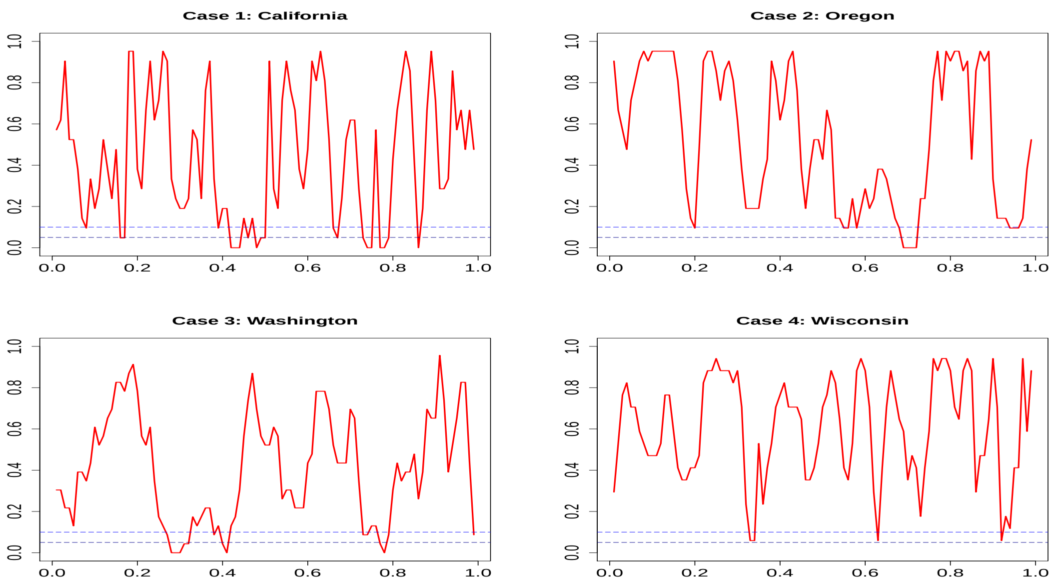


Figure A.13: The p-values of the quantile effects. The dashed lines are evaluated at the 5% and 10% levels.

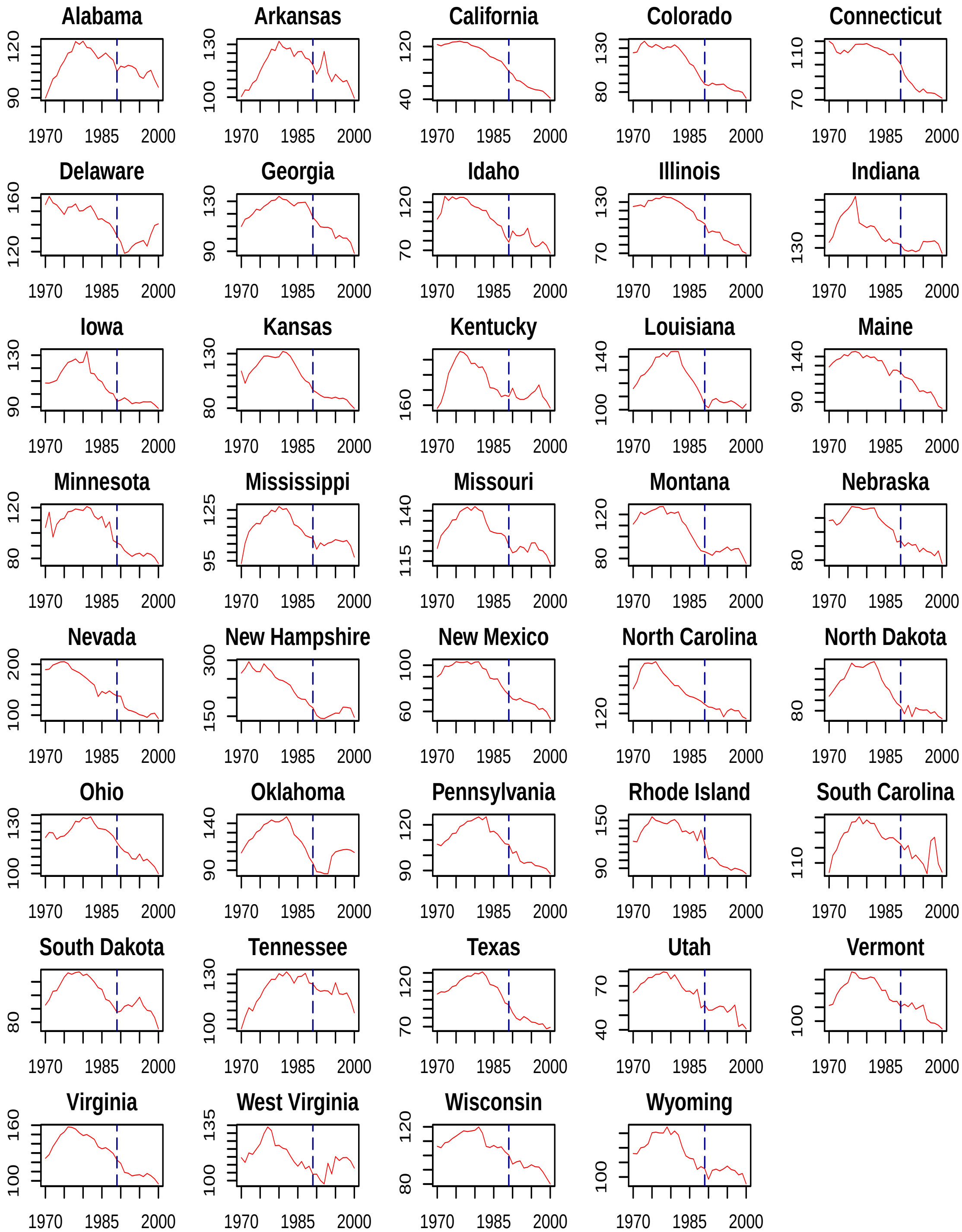


Figure A.14: Time series of per capita cigarette sales of California and 38 potential control states. The vertical dashed lines are evaluated at 1989.

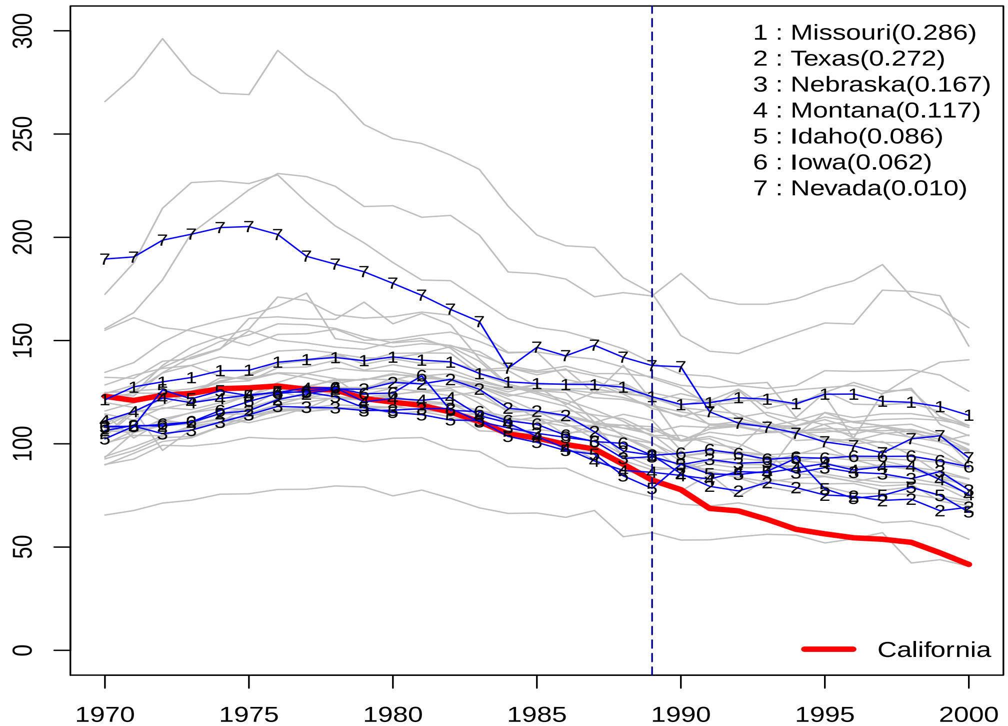


Figure A.15: Time series of per capita cigarette sales of California (red solid line), the first seven control states of the synthetic California generated by the proposed method (blue lines) and the remaining potential control states (gray lines). The numbers in parentheses are the synthetic-control weights. The vertical dashed lines are evaluated at 1989.

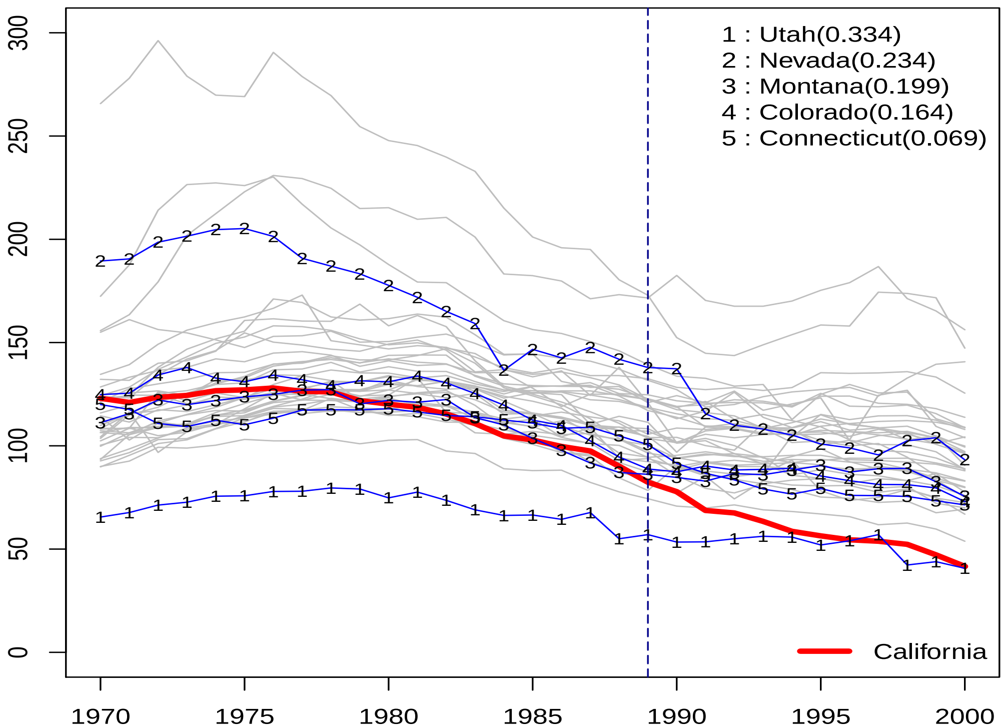


Figure A.16: Time series of per capita cigarette sales of California (red solid line), the first five control states of the synthetic California presented by ADH (blue lines) and the remaining potential control states (gray lines). The numbers in parentheses are the synthetic-control weights. The vertical dashed lines are evaluated at 1989.

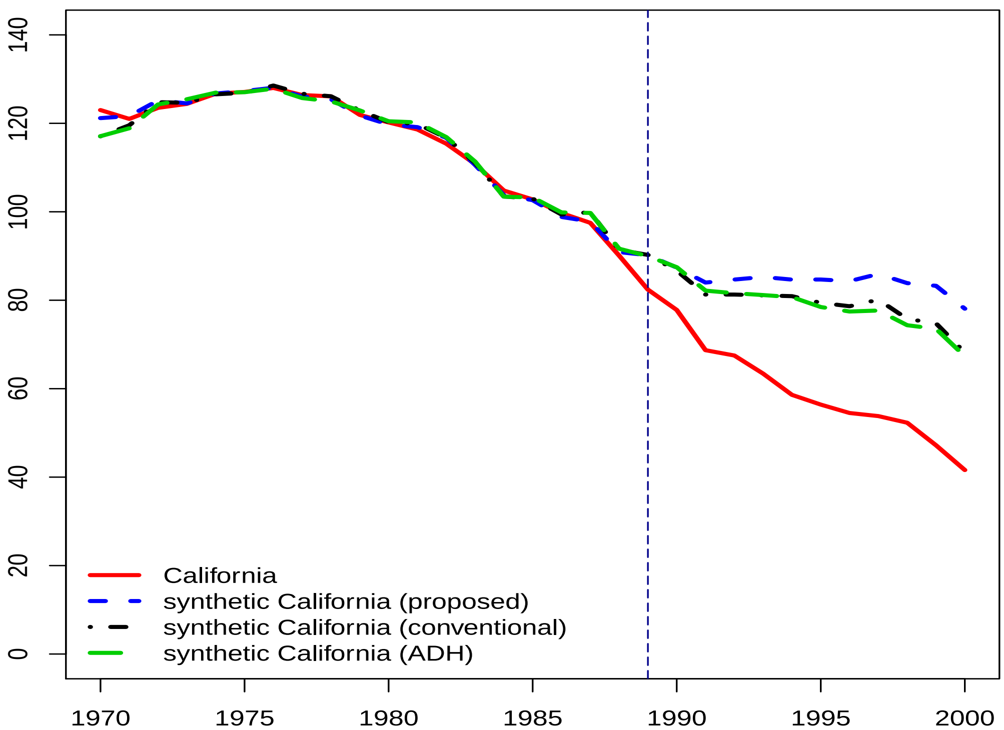


Figure A.17: Time series of per capita cigarette sales of California (red solid line), the synthetic California generated by the proposed method (blue dashed line), the synthetic California generated by the conventional method (black dashed and dotted line), and the synthetic California presented by ADH (green dashed line). The vertical dashed lines are evaluated at 1989.