Online Appendix to Hackmann, Kolstad, and Kowalski "Adverse Selection and an Individual Mandate: When Theory Meets Practice"

A.1 Plan Heterogeneity and the Extensive Margin

Our model addresses selection at the extensive margin and abstracts from intensive margin selection amongst differentiated plan. Our modeling decision follows naturally from the policy intervention, the individual mandate, which affects the demand for health insurance in general. While our framework may not be accurate in other contexts, we think that the modeling assumptions are sensible in this application for the following two reasons.

First, heterogeneity in plan generosity is limited in the Massachusetts individual health insurance market. According to Ericson and Starc (2012), 80% of the consumers in this market purchase bronze or silver plans, whose actuarial value varies between 60% and 70%.[1] We think that the variation at the intensive margin is small relative to having no health insurance at all.

Second, our modeling framework is consistent with plan heterogeneity if selection at the intensive margin is orthogonal to selection at the extensive margin. If so, we expect that the newly insured consumers purchase health insurance according to the observed market shares of the previously insured, holding the set of offered health insurance plans constant. In this case, we can aggregate heterogeneous plans to a single representative plan, which corresponds to a weighted average over the underlying individual plans, weighted by the plan market shares. Our framework models the willingness to pay and the costs of this representative plan.

While our data do not allow us to disentangle differences in preferences between the previously insured and the newly insured on the one hand from changes in plan generosity on the other hand, we notice that the actuarial value of the most popular plans changes only modestly between the pre- and the post-reform years, see Table 6 in the main paper. Hence, we conclude that on net, these effects do not seem to affect our empirical results considerably.

A.2 The Group Market and The Individual Market

Our model focuses on the individual market and abstracts from changes in the distribution of consumer types that may result from inflows from or outflows to the group market. For instance, Massachusetts health reform introduced an employer mandate, which encourages employers to offer health insurance to their employees, see Kolstad and Kowalski (2012). Hence, the reform may have created access to employer sponsored health insurance for at least some consumers that purchased health insurance individually in the pre-reform years.

Unfortunately, our data do not allow us to quantify the transition between the individual and the group market. The group market information in the SNL data is incomplete –it does not provide information on self-insured employers– and the NHIS sample population is too small to measure these transitions accurately.

[1] We are not counting silver plus and silver plus and silver select plans

Qualitatively, we interpret our welfare estimates as a conservative lower bound with respect to potential transitions from the individual to the group market for two reasons. First, if consumers switch to the group market, then we will understate the number of insured consumers in the individual market in the post-reform years, which biases our welfare estimates downwards. Second, we think that healthier individuals are more likely to be offered health insurance through their employers. Therefore, we will overstate the marginal costs of the newly insured. In other words, adverse selection would be more pronounced had the switchers remained in the individual market. Hence, the transition of inexpensive consumers to the group market biases our welfare estimates downward as well.

A.3 The Welfare Relevant Area

This section discusses the change in welfare caused by the elimination of adverse selection, which combines changes in consumer surplus, insurer surplus, and government surplus.

The consumer surplus corresponds to the integral over the difference between the willingness to pay and the market price for buyers minus the tax penalty payments made by the non-buyers. Using the notation from the consumer problem we can express the consumer surplus as:

$$CS(I^{*,t}, \Pi^t) = \int_0^{I^{*,t}} (D(x,0) - D(I^{*,t}, \Pi^t))dx - \Pi^t * (1 - I^{*,t}) + Y,$$

where we have substituted the equilibrium premium $P^(*, t)$ with the market level demand curve evaluated at the equilibrium coverage level and the respective tax penalty, $D(I^{*,t}, \Pi^t)$. Therefore, the change in consumer surplus between the pre-reform and the post-reform period is given by

$$\begin{aligned}\Delta CS &= CS(I^{*,post}, \Pi^{post} = \pi) - CS(I^{*,pre}, \Pi^{pre} = 0) \\ &= \int_{I^{*,pre}}^{I^{*,post}} D(x,0)dx - I^{*,post} * D(I^{*,post}, \pi) + I^{*,pre} * D(I^{*,pre}, 0) - \pi * (1 - I^{*,post}),\end{aligned}$$

which depends on the demand curve, the pre-reform and post-reform coverage levels, $I^{*,pre}$ and $I^{*,post}$, and the magnitude of the introduced penalty π. However, changes in consumer expenditures on health plan premiums, captured by the second and the third term, are not relevant for social welfare as they affect the insurer surplus through changes in revenues as well. Specifically, the insurer surplus refers to the integral over the difference between the market price and the marginal costs of the insured consumer. Therefore, the change in insurer surplus is given by

$$\begin{aligned}\Delta IS &= IS(I^{*,post}, \Pi^{post} = \pi) - IS(I^{*,pre}, \Pi^{pre} = 0) \\ &= I^{*,post} D(I^{*,post}, \pi) - I^{*,pre} D(I^{*,pre}, 0) - \int_{I^{*,pre}}^{I^{*,post}} MC(x)dx,\end{aligned}$$

which simply represents the difference between changes in revenues and changes in costs. Finally, the tax penalty payments increase government revenues. We assume that an extra dollar in government revenues adds ϕ to social welfare. For our empirical analysis, we assume $\phi = 1$ but generally ϕ may be smaller or greater than 1. Therefore, the overall change in welfare is given by

$$\Delta CS + \Delta IS + \Delta GS = \int_{I^{*,pre}}^{I^{*,post}} (D(x,0) - MC(x))dx - (1 - \phi) * \pi * (1 - I^{*,post}),$$

where ΔGS refers to the change in government surplus. Intuitively, the mandate increases welfare if the willingness to pay exceeds the marginal costs of the newly enrolled individuals. This welfare change can be visualized simply as a shaded region as shown in Figure 2 of the paper after we specify functional forms for the demand curve and the average cost curve. Furthermore, the mandate may reduce welfare if the raised tax penalty revenues do not contribute to social welfare at face value, i.e. $\phi < 1$.

A.4 Modeling Welfare

In this section, we derive equation 2 of the paper, which allows us to express the full welfare effect in terms of a set of measurable moments of the data.

The change in consumer surplus and provider profits, displayed in equation 2 of the paper, can be expressed by the primitives of the economic model, which will be particularly relevant in the empirical analysis. The full welfare effect is given by the combination of the light gray and the dark gray area, which equals the area underneath the old demand curve minus the area underneath the marginal cost curve bounded by $I^{*,pre}$ and $I^{*,post}$. We refer to these areas as *Area D* and *Area MC* respectively. Assuming linearity in demand, we can express the demand area as follows:

$$Area\ D = \frac{1}{2} * (P^{*,pre} - (P^{*,post} - \pi)) * (I^{*,post} - I^{*,pre}) + (P^{*,post} - \pi) * (I^{*,post} - I^{*,pre})\ .$$

Here, the first summand describes the triangle underneath the old demand curve, which is bounded by the equilibrium coverage levels and the post-reform premium minus the tax penalty. This adjusted post-reform premium marks the old willingness to pay evaluated at the post-reform coverage level. The second summand corresponds to the rectangle underneath the triangle, which is bounded by the coverage levels, the adjusted post-reform premium and the x-axis. The area underneath the marginal cost curve is simply the total change in variable costs, which can also be expressed as the difference between the post-reform and pre-reform product of average variable costs and coverage:

$$Area\ MC = AC^{*,post} * I^{*,post} - AC^{*,pre} * I^{*,pre}.$$

Combining and rearranging the terms we have:

$$\begin{aligned} \Delta W_{full} = Area\ D - Area\ MC \ &= \ (P^{*,pre} - AC^{*,pre}) * (I^{*,post} - I^{*,pre}) \\ &- \ (AC^{*,post} - AC^{*,pre}) * \Big(I^{*,pre} + (I^{*,post} - I^{*,pre})\Big) \\ &+ \ \frac{1}{2} * \Big((P^{*,post} - \pi) - P^{*,pre}\Big) * (I^{*,post} - I^{*,pre}) \end{aligned}$$

A.5 Linearity in Demand and the Welfare Effects

In this section, we assess the sensitivity of our baseline welfare effects with respect to the linearity assumption on the demand curve. Our baseline estimates equal 5.1% and 4.1% for the full and the net welfare effect respectively.

The data reveal two points on the old demand curve as indicated by point A and point C in Figure 8 of the paper. Therefore, we can calculate a lower bound and an upper bound of our full welfare effect by considering a L-shaped and an inverse L-shaped demand curve between points A and C. To construct the lower bound, we assume that the demand curve drops instantaneously to the post-reform level on the old demand curve and remains flat up until point C. Integrating the area between this L-shaped old demand curve and the marginal cost curve suggests a full welfare effect of -1.2%. This effect is, however, not statistically significant. To construct the upper bound, we assume that the demand curve remains flat between points A and C and drops to the to the post-reform level on the old demand curve at the post-reform coverage level. The revised demand curve raises the full welfare effect to 11.3%. Hence, the baseline estimate for the full welfare effect of 5.1% can be bounded by -1.2% from below and by 11.3% above if we allow for all possible downward sloping demand curves that go through points A and C.

Providing bounds for the net welfare effect is slightly more involved since we need to impose structure on the cost curves in order to calculate the interior coverage level at which the vertical difference between new demand curve and the average cost curve equals the pre-reform markup. We maintain the linearity assumption on the cost curves and construct bounds for different downward sloping demand curves. To construct an upper bound for the net welfare effect, we first notice that $I^{*,markup}$ converges to $I^{*,post}$ as the demand curve bends out from its linear form towards the inverse L-shaped form, see Figure 8 of the paper. In the limit, we have $I^{*,markup} = I^{*,post}$ and $W^{net} = W^{full} = 11.3\%$. To construct the lower bound, we notice that $I^{*,markup}$ converges to $I^{*,pre}$ as the demand curve bends in from its linear form towards the L-shaped form. In the limit, the net welfare effect converges to 0% as the coverage gain $I^{*,markup} - I^{*,pre}$ converges to 0. Hence, the baseline estimate for the net welfare effect of 4.1% can be bounded by 0% from below and by 11.3% from above.

A.6 Post-Reform Coverage Under Pre-Reform Markup

In this section, we derive the formula for the post-reform coverage level under the pre-reform markup, see equation 4 of the paper.

To find the post-reform coverage level under the pre-reform markup, we set the post-reform demand curve equal to the average cost curve plus the pre-reform markup. In our linearized framework, we can express these curves as follows:

$$D(I, \pi) = \alpha_0 + \alpha_1 * I + \pi$$

$$AC(I) + load^{*,pre} = \beta_0 + \beta_1 * I + P^{*,pre} - AC^{*,pre}.$$

Here, α_0 and β_0 are intercept terms and α_1 and β_1 are the respective slope terms. Solving for coverage I, we find:

$$I^{*,markup} = \frac{\beta_0 - \alpha_0 + P^{*,pre} - AC^{*,pre}}{\alpha_1 - \beta_1} - \pi * \frac{1}{\alpha_1 - \beta_1}.$$

We also now that $I^{*,markup} = I^{*,pre}$ for $\pi = 0$. Therefore, we have:

$$I^{*,markup} = I^{*,pre} + \pi * \frac{I^{*,post} - I^{*,pre}}{(AC^{*,post} - AC^{*,pre}) - ((P^{*,post} - \pi) - P^{*,pre})} \quad .$$

A.7 Optimal Coverage And Optimal Penalty

In this section, we derive the formulas for optimal coverage and the optimal tax penalty displayed in equations 5 and 6 of the paper respectively.

To find the optimal insurance coverage we first consider an interior solution that corresponds to the intersection of the pre-reform demand curve and the marginal cost curve. Using the notation from the previous section, we find that:

$$\alpha_0 + \alpha_1 * I = \beta_0 + 2 * \beta_1 * I$$

$$\iff \quad I^{*,opt} = \frac{\beta_0 - \alpha_0}{\alpha_1 - 2\beta_1}$$

Adding and subtracting $P^{*,pre} - MC^{*,pre}$ in the numerator we find that:

$$\begin{aligned} I^{*,opt} &= \frac{\beta_0 - \alpha_0 + (P^{*,pre} - MC^{*,pre})}{\alpha_1 - 2\beta_1} - \frac{(P^{*,pre} - MC^{*,pre})}{\alpha_1 - 2\beta_1} \\ &= I^{*,pre} + \frac{(P^{*,pre} - MC^{*,pre}) * (I^{*,post} - I^{*,pre})}{2(AC^{*,post} - AC^{*,pre}) - ((P^{*,post} - \pi) - P^{*,pre})} \quad . \end{aligned}$$

Adding and subtracting $AC^{*,pre} * (I^{*,post} - I^{*,pre})$ to the numerator of the ratio, we can rewrite the second term as:

$$\frac{(P^{*,pre} - AC^{*,pre}) * (I^{*,post} - I^{*,pre})}{2(AC^{*,post} - AC^{*,pre}) - ((P^{*,post} - \pi) - P^{*,pre})} + \frac{(AC^{*,pre} - MC^{*,pre}) * (I^{*,post} - I^{*,pre})}{2(AC^{*,post} - AC^{*,pre}) - ((P^{*,post} - \pi) - P^{*,pre})}$$

and using the linearity of the average cost curve, we have:

$$AC^{*,pre} - MC^{*,pre} = -\frac{AC^{*,post} - AC^{*,pre}}{I^{*,post} - I^{*,pre}} * I^{*,pre} \; .$$

Finally, we consider that the optimal coverage is bounded from below and above by one and zero respectively. Combining these terms, we find that the optimal insurance coverage can be expressed as shown in equation 5 of the paper. The optimal tax penalty shifts the equilibrium coverage level to the optimum. To find this penalty, we set the post-reform demand curve, evaluated at the optimal coverage level, equal to the average cost plus the post reform markup:

$$D(I^{*,opt}, \pi) = AC(I^{*,opt}) + P^{*,post} - AC^{*,post} \; .$$

and solve this equation for π. We have:

$$\begin{aligned} \alpha_0 + \alpha_1 * I^{*,opt} + \pi &= \beta_0 + \beta_1 * I^{*,opt} + P^{*,post} - AC^{*,post} \\ \iff \quad P^{*,pre} + \alpha_1(I^{*,opt} - I^{*,pre}) + \pi &= AC^{*,pre} + \beta_1(I^{*,opt} - I^{*,pre}) + P^{*,post} - AC^{*,post} \\ \iff \quad \pi^{*,opt} &= (P^{*,post} - P^{*,pre}) - (AC^{*,post} - AC^{*,pre}) \\ &+ \frac{AC^{*,post} - AC^{*,pre} - ((P^{*,post} - \pi) - P^{*,pre})}{I^{*,post} - I^{*,pre}} * (I^{*,opt} - I^{*,pre}) \end{aligned}$$

A.8 Synthetic Control Weights

Table A1 displays the weights of each control state in the empirical analysis. We use the same weights to estimate the effect of Massachusetts health reform on insurance coverage, log average costs, and log premiums. Table A1 reports a missing value for those states that are excluded from the empirical analysis, see the data section 5 for details. While all of the remaining 34 control states receive a positive weight, it is evident that Maine, Vermont, and North Dakota receive the highest weights. Interestingly, Maine and Vermont had comparable guaranteed issue and community rating regulations in place. We revisit the role of states with guaranteed issue and community rating regulations as potential control states in appendix section A.12.

A.9 Demographic and Economic Trends

In this section, we assess the sensitivity of our main estimates with respect to concurrent demographic and economic trends in the sample period. We do not advocate controlling for characteristics of enrollees in the main specifications because doing so could obscure real impacts of reform. Following EFC, the characteristics of the enrollees ultimately drive coverage, costs, and premiums, but because insurers cannot price based on them, it does not make sense to hold them fixed.

Table A1: Synthetic Control Weights

State	Synthetic Control Weights	State	Synthetic Control Weights
Alabama	-	Montana	0.019
Alaska	-	Nebraska	0.013
Arizona	0.007	Nevada	0.007
Arkansas	0.014	New Hampshire	0.007
California	-	New Jersey	0.038
Colorado	0.007	New Mexico	0.052
Connecticut	0.004	New York	0.009
Delaware	-	North Carolina	0.006
District of Columbia	-	North Dakota	0.178
Florida	0.004	Ohio	-
Georgia	0.007	Oklahoma	-
Hawaii	-	Oregon	0.01
Idaho	0.006	Pennsylvania	0.007
Illinois	-	Rhode Island	0.007
Indiana	-	South Carolina	0.008
Iowa	-	South Dakota	-
Kansas	-	Tennessee	0.005
Kentucky	0.007	Texas	0.003
Louisiana	0.018	Utah	0.017
Maine	0.226	Vermont	0.235
Maryland	0.01	Virgina	-
Michigan	0.008	Washington	0.006
Minnesota	0.005	West Virgina	-
Mississippi	-	Wisconsin	0.006
Missouri	0.027	Wyoming	0.018

However, from an empirical standpoint, it might be interesting to examine whether broad demographic trends in the entire population (and not just the enrolled population) drive our results. To this end, we add controls for demographic and economic characteristics at the state-year level to our primary empirical equation. Using data from the American Community Survey (ACS), we construct demographic and economic variables for the non-elderly adult population aged 18-64. Unfortunately, the ACS time series begins in 2005, which is why we treat the 2005 measures as pre-reform characteristics and simply extrapolate this information to the year 2004.[2]

Table A2 compares the baseline estimates in columns 1,3, and 5 with the estimates from the extended regression specification in columns 2,4, and 6. The main effects of interest are displayed in the first row. The results are very similar in magnitude and suggest, if anything, larger effects on coverage, log average costs, log premiums, and ultimately on social welfare. Based on this evidence, we conclude that our primary estimates are robust to the inclusion of additional statewide demographic and economic variables.

[2] One alternative would be to use information from the decennial census. In this case, we would have to extrapolate based on information in 2000, which is potentially less accurate for 2004 than the information in 2005.

Table A2: Demographic and Economic Trends

	(1)	(2)	(3)	(4)	(5)	(6)
	Coverage	Coverage	Log Premium	Log Premium	Log Claim Exp	Log Claim Exp
MA*After	0.265***	0.340***	-0.233***	-0.235***	-0.087***	-0.124*
	[0.175, 0.362]	[0.198, 0.485]	[-0.286, -0.176]	[-0.309, -0.147]	[-0.143, -0.025]	[-0.214, 0.015]
MA*During	-0.030*	0.009	-0.012	-0.032	-0.019	-0.063
	[-0.066, 0.003]	[-0.098, 0.122]	[-0.063, 0.036]	[-0.092, 0.043]	[-0.076, 0.038]	[-0.152, 0.047]
MA	0.112*	0.229	0.700***	0.430**	0.761***	0.544*
	[-0.010, 0.238]	[-0.132, 0.711]	[0.622, 0.779]	[0.052, 0.833]	[0.662, 0.870]	[-0.073, 0.994]
After	-0.044	0.181**	0.128***	-0.009	0.213***	0.076
	[-0.137, 0.045]	[0.038, 0.364]	[0.073, 0.181]	[-0.132, 0.122]	[0.150, 0.268]	[-0.095, 0.232]
During	-0.003	0.021	0.087***	0.060	0.156***	0.106*
	[-0.036, 0.032]	[-0.110, 0.124]	[0.041, 0.138]	[-0.031, 0.177]	[0.099, 0.212]	[-0.011, 0.272]
Share 18-24		0.561		-6.935***		-4.868
		[-6.868, 4.424]		[-10.465, -1.935]		[-9.811, 2.152]
Share 25-34		0.871		-9.181**		-7.967
		[-9.617, 6.442]		[-14.639, -0.284]		[-13.594, 4.053]
Share 35-44		7.277**		-7.898***		-8.959**
		[1.665, 14.199]		[-12.488, -2.392]		[-16.124, -1.167]
Share 45-54		-2.849		-8.660**		-4.370
		[-14.779, 3.814]		[-15.693, -0.690]		[-10.980, 3.220]
Share Women		-0.577		-1.231		0.610
		[-9.358, 6.551]		[-7.887, 7.282]		[-7.123, 12.447]
Share Black		0.029		0.528		0.855
		[-6.607, 1.741]		[-2.275, 3.889]		[-2.529, 5.103]
Share White		-0.244		1.180*		1.856*
		[-7.202, 0.794]		[-0.380, 4.487]		[-0.372, 6.278]
Share Asian		7.315		3.486		5.946
		[-5.385, 26.936]		[-7.949, 12.434]		[-8.548, 18.162]
Share Unemployed		-3.902*		-0.957		-3.555
		[-8.530, 0.410]		[-3.643, 3.106]		[-6.631, 1.291]
Avg. Wage		-0.000***		0.000**		0.000
		[-0.000, -0.000]		[0.000, 0.000]		[-0.000, 0.000]
Constant	0.591***	1.549	7.978***	13.188***	7.808***	10.246
	[0.468, 0.711]	[-3.153, 13.270]	[7.899, 8.057]	[3.287, 19.285]	[7.699, 7.907]	[-3.654, 17.616]

The bootsrapped 95% confidence interval is displayed in brackets.

Standard errors are block bootstrapped. Abadie weights depend on member month enrollment as well as changes in coverage, relative changes in average costs, and relative changes in premiums between 2004 and 2005. The additional control variables refer to the population of nonelderly adults aged 18-64.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.10 Placebo Analysis

In this section, we conduct a placebo analysis in which we replace Massachusetts with a set of plausible control states as though they were treated. Specifically, we conduct three exercises. Following Abadie et al. (2010), we first revisit the analysis by replacing Massachusetts with placebo states whose pre-reform trends can be reasonably matched by respective control states. Second, following Abadie and Gardeazabal (2003), we revisit the analysis by replacing Massachusetts with placebo states that are similar to Massachusetts. Finally, we compare post-reform deviations to pre-reform deviations from the respective control state trends between Massachusetts and all other states. This approach has been advocated by Abadie et al. (2010).

Following Abadie et al. (2010), we first construct control states for each placebo state by matching the pre-reform trends in coverage, log average costs, and log premiums as well as pre-reform health insurance enrollment levels in the placebo state. Second, we evaluate the quality of the pre-reform match by constructing the mean-squared prediction error (MSPE) for our key outcome variables in the year 2006, when the health reform has not had an effect yet. Specifically, we first construct the MSPE for each outcome variable. Second, we normalize the measure by the respective MSPE in Massachusetts, and third, we sum the MSPEs across the three outcome variables. Finally, we construct our pre-reform MSPE criterion by dividing the normalized placebo state-specific total MSPE by the total MSPE in Massachusetts. This pre-reform MSPE criterion summarizes the quality of the match relative to Massachusetts. Following Abadie et al. (2010), we only focus on those placebo states whose pre-reform trends can be reasonably matched by potential control states. We have used their proposed MSPE cutoff of 2 to select the relevant placebo states. These are Arizona, Nebraska, and North Dakota. Figures A1, A2, and A3 contrast the experience in Massachusetts with those in the plausible placebo states.

Figure A1: Placebo Analysis for Insurance Coverage (Based on Pre-Reform MSPE Criterion)

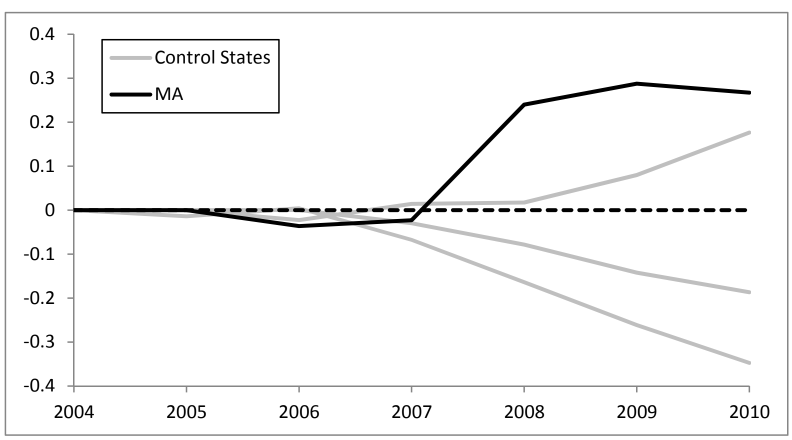


Figure A2: Placebo Analysis for Log Premiums (Based on Pre-Reform MSPE Criterion)

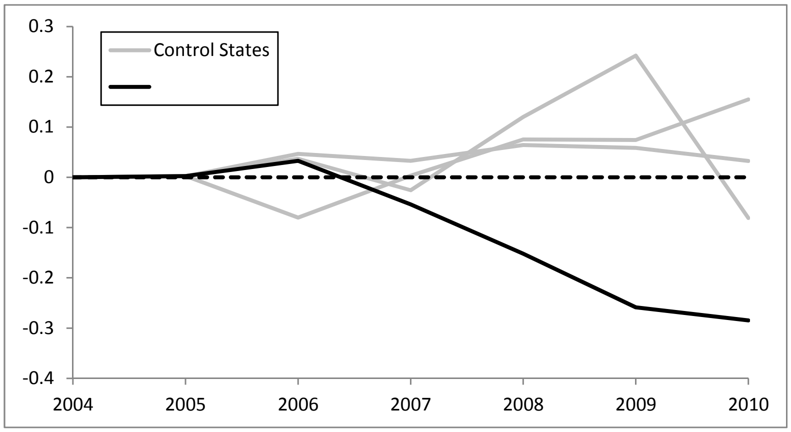
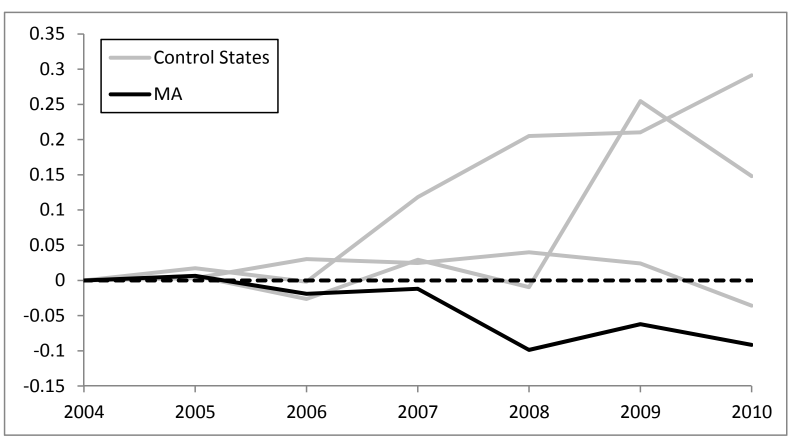


Figure A3: Placebo Analysis for Log Average Costs (Based on Pre-Reform MSPE Criterion)



These graphs document that the experience in Massachusetts was distinctively different from those in the placebo states.

Another criterion to select plausible placebo states is to consider those states that are very similar to Massachusetts – states that receive high synthetic control weights. This approach has been used in Abadie and Gardeazabal (2003) who conduct a placebo test for the region that receives the highest weight in the baseline analysis: Catalonia. In our application, Maine, North Dakota, and Vermont receive by far the largest weights. Therefore, we contrast Massachusetts' trends with trends in these three states in Figures A4, A5, and A6.

Figure A4: Placebo Analysis for Insurance Coverage (Using Similar States)

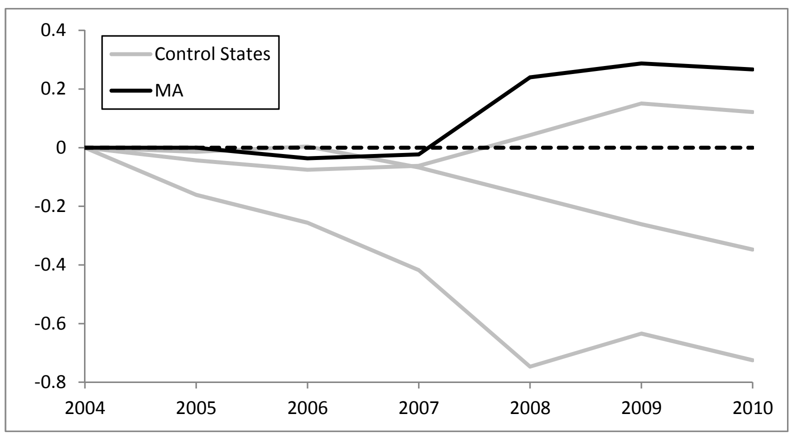


Figure A5: Placebo Analysis for Log Premiums (Using Similar States)

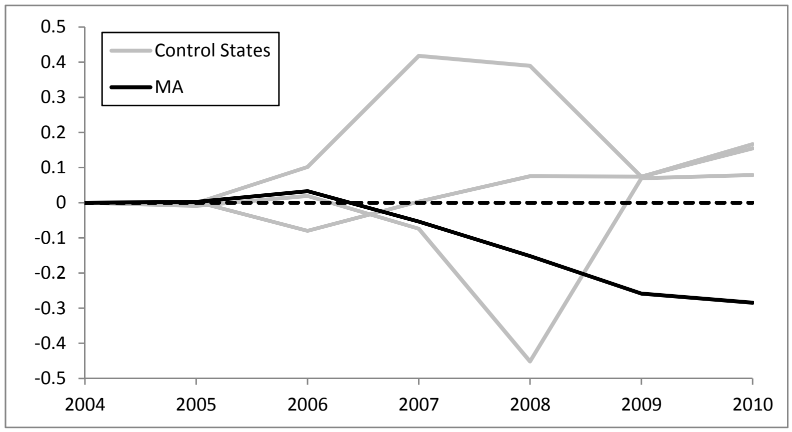
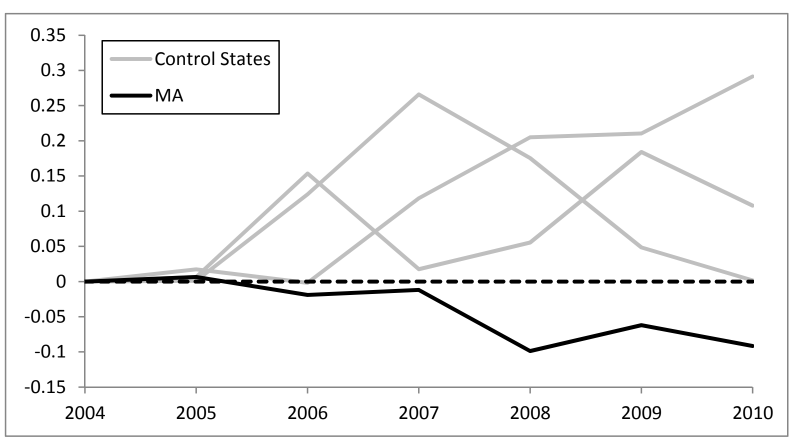
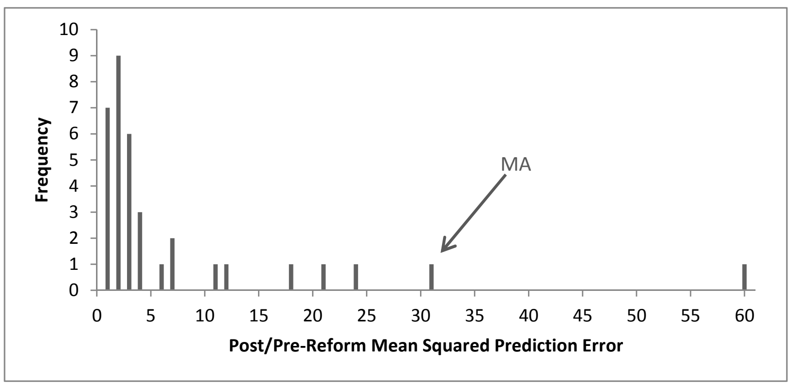


Figure A6: Placebo Analysis for Log Average Costs (Using Similar States)



Again, these graphs document that the experience in Massachusetts was distinctively different from those in the placebo states. Finally, again following Abadie et al. (2010), we have also constructed the post/pre-reform MSPE-ratio. This measure, which contrasts the MSPE in the post-reform year 2008 with the discussed MSPE in 2006, provides a measure of how poorly the pre-reform match fits the post-reform data, relative to the pre-reform period. The idea behind this statistic is that a high post/pre-reform MSPE-ratio indicates that after the reform, the trends in the given state have moved away from the trends predicted in the pre-period data. Hence, a high post/pre-reform MSPE-ratio is indicative of an effect of the reform. The histogram in figure displays the post/pre-reform MSPE-ratio for all states.

Figure A7: Placebo Analysis: Post/Pre-reform MSPE Criterion



Massachusetts has the second highest post/pre-reform MSPE-ratio, lagging only behind New Jersey. However, it appears that there may be data issues in the New Jersey reporting in 2008. The data suggest that average costs as well as premiums have fallen by more than 50% in the major carrier between 2006 and 2007 despite the fact that inpatient admissions per enrollee have increased and the average number hospital days has remained fairly constant in this carrier. While New Jersey is included in the main regressions, it has a relatively low weight, so the data issue in 2008 is unlikely to impact our main results. If anything, though, it would bias our findings down.

A.11 Proportional Demand Shifts in the Logarithmic Model

In our empirical analysis, we assume that the old demand curve is loglinear, that is

$$log(P) = \alpha_0 + \alpha_1 * I \ .$$

Now we consider the effects of a relative tax penalty of $\pi = \frac{\$1{,}250}{P^{*,pre}}$, where we divide the tax penalty by the pre-reform premium in Massachusetts. The new demand curve equals:

$$log(P - \pi * P) = \alpha_0 + \alpha_1 * I \ .$$

Rearranging terms, we see that the proportional tax penalty implies a parallel shift of the old demand curve as shown in Figures 2 and 3 of the paper:

$$log(P) = -log(1 - \pi) + \alpha_0 + \alpha_1 * I \ .$$

The magnitude of the demand shift is given by $-log(1 - \pi) > 0$ for $0 < \pi < 1$. With respect to the optimal parallel shift suggested by equation 6 of the paper, we see that this shift of magnitude x can be induced by a tax penalty of $1 - \exp(-x)$.

A.12 Synthetic Control Weights with Focus on Guaranteed Issue States

Table A3 displays the revised underlying weights that emphasize the role of important pre-reform regulations in Massachusetts: guaranteed issue and community rating. These weights were generated by adding an indicator for states with these regulations to the weight-generating algorithm, alongside the variables used to generate the baseline weights. Compared with the weights in the baseline analysis shown in Table A1, we see that this alternative approach continues to place a high weight on Maine and Vermont, but it also places much more weight on New York, a third state which had guaranteed and community rating regulations in place. The approach also assigns positive weight to Connecticut and Texas, which did not have these regulations in place, in order to match other key characteristics of pre-reform period: changes in coverage, changes in log average costs, changes in log premiums, and health insurance enrollment levels.

A.13 Trends Using Synthetic Control Weights with Focus on Guaranteed Issue States

Figures A8, A9, and A10 show trends in enrollment, premiums, and claim expenditures in Massachusetts relative to the other states, adding more weight to states with guaranteed issue and community rating regulations. The trends in the control states look very similar to the national trends displayed in Figures 5, 6, and 7 of the paper.

Table A3: Synthetic Control Weights with Focus on Guaranteed Issue States

State	Synthetic Control Weights	State	Synthetic Control Weights
Alabama	-	Montana	0
Alaska	-	Nebraska	0
Arizona	0	Nevada	0
Arkansas	0	New Hampshire	0
California	-	New Jersey	0
Colorado	0	New Mexico	0
Connecticut	0.099	New York	0.206
Delaware	-	North Carolina	0
District of Columbia	-	North Dakota	0
Florida	0	Ohio	-
Georgia	0	Oklahoma	-
Hawaii	-	Oregon	0
Idaho	0	Pennsylvania	0
Illinois	-	Rhode Island	0
Indiana	-	South Carolina	0
Iowa	-	South Dakota	-
Kansas	-	Tennessee	0
Kentucky	0	Texas	0.017
Louisiana	0	Utah	0
Maine	0.308	Vermont	0.37
Maryland	0	Virgina	-
Michigan	0	Washington	0
Minnesota	0	West Virgina	-
Mississippi	-	Wisconsin	0
Missouri	0	Wyoming	0

Figure A8: Insurance Coverage Amongst GI States

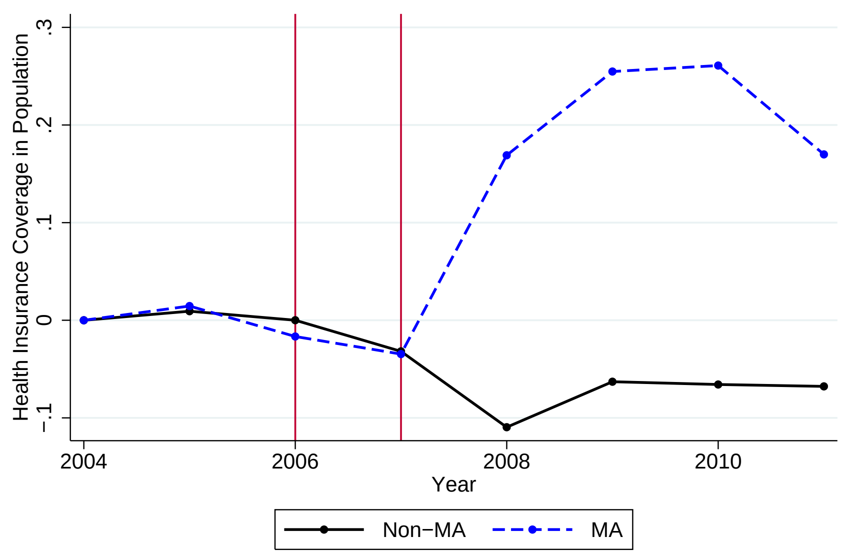


Figure A9: Annual Premiums Amongst GI States

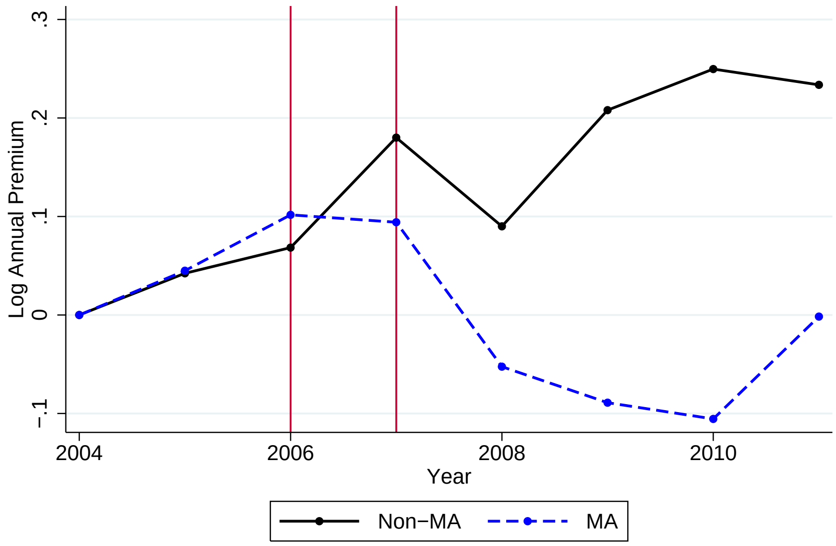
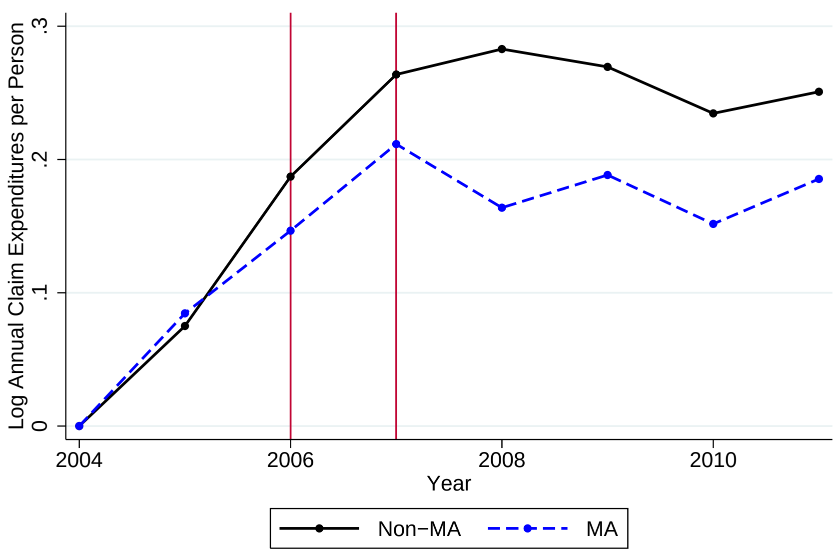


Figure A10: Annual Average Claim Expenditures Amongst GI States



B Appendix: Bootstrap

To assess the precision of our welfare estimates, we derive the distribution of the welfare effects via bootstrap. We proceed as follows. To incorporate the constructed synthetic control weights, we first expand the state-year observations proportionally. Using the constructed weights, we expand the data by a factor of 1,000 such that the sequence of years, for each state, is represented in the full sample according to the constructed synthetic control weights. The synthetic control method constructs weights for the control states only, which add up to one. For Massachusetts, we simply take the ratio of one divided by the number of states in the empirical analysis.

In a second step, we apply a block bootstrap approach to the SNL data and draw entire state clusters with replacement from the expanded state-year level sample. The number of draws equals the number of states in the original sample, prior to the expansion.

We also consider the statistical variation in the NHIS coverage estimates in our bootstrap approach, which are used to normalize the enrollment measures in the SNL data. To this end, we draw post-reform coverage levels for Massachusetts and the other states. Specifically, for each state within the sample, we draw N_{state} observations from a Bernoulli distribution with mean μ_{state} and construct the arithmetic mean. Here, N_{state} and μ_{state} refer to the number of observations and the coverage level in the post-reform NHIS sample for the given state respectively. We use the respective post-reform coverage levels to normalize the enrollment trends in the drawn sample.

Third, we conduct the relevant difference-in-differences regressions and save the estimated pre-reform levels in Massachusetts and the estimates for γ^k (note that we use the exact same sample to estimate the three equations). If the drawn sample does not include Massachusetts, then we still use the sample to estimate the non-Massachusetts parameters.[3] Notice that these are unweighted regressions because the sample has been expanded to incorporate the weights. We calculate the full and the net welfare effect using the outlined formulas. Finally, we report the 2.5, and the 97.5 percentile of the estimated welfare effect distributions. We repeat this procedure for different penalty values and report the results in Table 3 of the paper.

B.1 Alternative Method

In this section, we revisit our baseline inference method and consider a more parametric approach that allows us to model contemporaneous shocks in Massachusetts. We consider the following model

$$Y_t^{k,s} = \begin{cases} \tau^{k,reform} + \epsilon_t^{k,s} & if\ treated \\ \epsilon_t^{k,s} & otherwise \end{cases}$$

where $Y_t^{k,s}$ denotes outcome k in state s in post-reform year t from 2008-2010. The outcomes of interest are insurance coverage, log claim expenditures, and log premiums respectively. $\tau^{k,reform}$ captures the reform effects of interest. Our baseline estimator for the reform effect is given by

$$\hat{\tau}^{k,reform} = \frac{1}{T^{post}} \sum_t Y_t^{k,MA} - \frac{1}{T^{post}} \sum_t \sum_{i \neq MA} w_i Y_t^{k,i}$$

where T^{post} measures the number of post-reform years and w_i refers to the synthetic control weights discussed in the main text. We assume that the shocks $\epsilon_t^{k,s}$ are independent across states and over

[3] These include the constant term, ρ_5^k, the during effect, ρ_4^k, and the after effect, ρ_3^k.

time. These assumptions imply the following variance for our baseline estimator:

$$\begin{aligned} var(\hat{\tau}^{k,reform}) &= var\Big(\frac{1}{T^{post}}\sum_t(\tau^{k,reform}+\epsilon_t^{k,MA})+var(\frac{1}{T^{post}}\sum_t\sum_{i\neq MA}w_i\epsilon_t^{k,i}\Big) \\ &= \Big(\frac{1}{T^{post}}\Big)^2\sum_t var(\epsilon^{k,MA})+\Big(\frac{1}{T^{post}}\Big)^2\sum_t var(\sum_{i\neq MA}w_i\epsilon^{k,i}) \\ &= \frac{1}{T^{post}}*var(\epsilon^{k,MA})+\frac{1}{T^{post}}*var(\sum_{i\neq MA}w_i\epsilon^{k,i}) \end{aligned}$$

To estimate the variance of our estimator, we estimate the second variance component of the previous equation as follows:

$$v\hat{a}r(\sum_{i\neq MA}w_i\epsilon^{k,i})=\frac{1}{T^{post}}\sum_t(\sum_{i\neq MA}w_i\hat{\epsilon_t}^{k,i})^2-\Big(\frac{1}{T^{post}}\sum_t(\sum_{i\neq MA}w_i\hat{\epsilon_t}^{k,i})\Big)^2\ , \tag{1}$$

where $\hat{\epsilon_t}^{k,i}$ refers to the estimated state-year specific shock. To estimate the first variance component, we employ the observed shocks in the control states weighted by the synthetic control weights:

$$v\tilde{\hat{a}}r(\epsilon^{k,MA})=\sum_{i\neq MA}w_i var(\hat{\epsilon}^{k,i})=\sum_{i\neq MA}w_i\Big(\frac{1}{T^{post}}\sum_t(\hat{\epsilon_t}^{k,i})^2-(\frac{1}{T^{post}}\sum_t\hat{\epsilon_t}^{k,i})^2\Big)\ .$$

In the following analysis, we use a more conservative estimator:

$$v\hat{a}r(\epsilon^{k,MA})=\sum_{i\neq MA}w_i\Big(\frac{1}{T^{post}}\sum_t(\hat{\epsilon_t}^{k,i})^2-(\frac{1}{T}\sum_t\hat{\epsilon_t}^{k,i})^2\Big) \tag{2}$$

which, intuitively, exploits variation in shocks around the state-specific sample mean and not just around the state-specific post-reform mean.

B.1.1 Results

To estimate the shocks displayed above, we estimate our baseline regression model from the main text including state fixed effects. The inclusion of state fixed effects does not alter the point estimates but normalizes the residuals to zero at the state level, which allows us to ignore the second summand in equation (2). Since our baseline specification includes a post-reform fixed effect, the average residual across states and across post-reform years equals zero as well. Hence, we can ignore the second summand in equation (1) as well. That means that we can construct the standard deviation of our estimator as follows:

$$sd^k=\sqrt{\frac{1}{T^{post}}}*\sqrt{\sum_{i\neq MA}w_i\Big(\frac{1}{T^{post}}\sum_t(\tilde{\epsilon}^{k,i})^2\Big)+\frac{1}{T^{post}}\sum_t(\sum_{i\neq MA}w_i\tilde{\epsilon}^{k,i})^2}$$

where $\tilde{\epsilon}^{k,i}$ denotes the residual from the baseline regression model including state fixed effects.

Finally, we construct 95 percent confidence intervals by starting with the point estimate and adding and subtracting 1.96 times the estimated standard deviation. We present these confidence intervals in the two panels of Table B1, which differ in the underlying synthetic control weights. The top panel displays point estimates and confidence intervals for our baseline synthetic control weights. Immediately beneath the point estimates, we present confidence intervals from the alternative parametric approach introduced here. For comparison, immediately below those confidence intervals in the rows labeled "Bootstrapped Percentiles," we report confidence intervals from our main block bootstrap approach. For yet another comparison, in the rows labeled "Bootstrapped SD" we also report *symmetric* confidence intervals from our block bootstrap approach, which we obtain by starting with the point estimate and adding and subtracting 1.96 times the standard deviation of the bootstrap reps, instead of reporting quantiles of the bootstrap reps. As shown, the confidence intervals from both bootstrapped procedures are very similar, regardless of whether they are constructed using the "percentile" or "SD" approach. The confidence intervals from the revised inference approach add another term to those from the "boostrapped SD" approach.

As expected, the confidence intervals from the revised inference approach are slightly larger because they account for contemporaneous shocks in Massachusetts. Our estimated reform effects on enrollment and log premiums remain statistically significant at the 1 percent level.

Table B1: Revised Confidence Intervals

	(1)	(2)	(3)
Baseline:	Coverage	Log Premium	Log Claim Exp
γ^k MA*After	0.265***	-0.233***	-0.087*
	[0.098, 0.432]	[-0.419, -0.046]	[-0.186, 0.011]
Bootstrapped Percentiles:	[0.175, 0.362]	[-0.286, -0.176]	[-0.143, -0.025]
Bootstrapped SD:	[0.172, 0.358]	[-0.288, -0.178]	[-0.146, -0.029]

	(1)	(2)	(3)
Guaranteed Issue:	Coverage	Log Premium	Log Claim Exp
γ^k MA*After	0.305***	-0.266**	-0.099**
	[0.138, 0.472]	[-0.453, -0.080]	[-0.197, -0.001]
Bootstrapped Percentiles:	[0.205, 0.402]	[-0.319, -0.213]	[-0.157, -0.033]
Bootstrapped SD:	[0.207, 0.403]	[-0.318, -0.214]	[-0.161, -0.037]

The 95% confidence intervals are displayed in brackets.

The top panel compares confidence intervals for the key parameters of interest based on our baseline Abadie weights. The bottom panel compares the respective confidence intervals when the Abadie weights also take guaranteed issue and community rating regulations into account. The second row in each panel displays the confidence intervals based on the parametric approach discussed in Section B1. The third row displays the block bootstrapped confidence inrervals of the baseline analysis. The fourth row displays the confidence intervals based on block bootstrapped standard deviations.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The lower panel displays the analogous results based on synthetic control weights that also take guaranteed issue and community rating regulations into account. Again, the confidence intervals of our revised approach are very similar but slightly larger than those derived from the two block bootstrap approaches. Based on the revised confidence intervals, our estimated reform effects on enrollment and log premiums remain statistically significant at the 1 percent level. The effect on log claim expenditures is significant at the 5 percent level. Based on the evidence presented in Table B1, we conclude that our main findings are robust to alternative block bootstrap and parametric inference approaches.

References

Alberto Abadie and Javier Gardeazabal. The economic costs of conflict: A case study of the basque country. *American Economic Review*, 93(1):113–132, March 2003.

Alberto Abadie, Alexis Diamond, and Jens Hainmueller. Synthetic control methods for comparative case studies: Estimating the effect of californias tobacco control program. *Journal of the American Statistical Association*, 105(490), 2010.

Keith M. Marzilli Ericson and Amanda Starc. Pricing regulation and imperfect competition on the massachusetts health insurance exchange. *NBER Working Paper 18089*, 2012.

Jonathan T Kolstad and Amanda E Kowalski. Mandate-based health reform and the labor market: Evidence from the massachusetts reform. *NBER Working Paper 16012*, 2012.