Supplementary Online Content


**eAppendix.** Identification Strategy

**eTable 1.** Definition of Eligible and Ineligible Children in Mandate and Nonmandate States

**eTable 2.** Treated Prevalence

This supplementary material has been provided by the authors to give readers additional information about their work.
eAppendix. Identification Strategy

We sought to evaluate the impact of state autism mandates via a difference-in-differences (DiD) strategy comparing treated prevalence in four groups of child-month observations that differed along two dimensions: (1) by the child’s location in a state in a month when a mandate was or was not active, and (2) by the child’s eligibility to be covered by the mandate based on the source of the child’s health insurance and age. Membership in these groups is described in eTable 1.

It is important to note that our approach requires an assumption about the age criteria for coverage among states that did not implement mandates prior to 2013. As the modal state mandate law covered children 0 to 21 years old, we assumed that children in states without mandates would have been covered by a counterfactual mandate if they were in fully insured health plans and they were 0 to 21 years old. Below, we discuss a sensitivity test in which eligibility was limited to children 0 to 18 years old in non-mandate states.

Model Specification and Estimation

The estimating equation for the unadjusted DiD model is:

\[
\logit[P(Y_{ist} = 1)] = a + bM mandate_{ist} + cEligible_{ist} + d(M mandate_{ist} \times Eligible_{ist}) + \varepsilon_{ist}
\]

where \(Y_{ist}\) is an indicator for whether child-month \(i\) in state \(s\) in year \(t\) has at least one claim for an autism, \(M mandate_{ist}\) is an indicator for whether state \(s\) had an active mandate in year \(t\), and \(Eligible_{ist}\) is an indicator for whether child-month \(i\) was (or would have been) eligible to be covered by the mandate.

The estimating equation for the adjusted DiD model is:

\[
\logit[P(Y_{ist} = 1)] = a + bM mandate_{ist} + cEligible_{ist} + d(M mandate_{ist} \times Eligible_{ist}) + fX_{ist} + \gamma_s + \delta_t + g(\gamma_s \times \delta_t) + h(\gamma_s \times Eligible_{ist}) + i(\delta_t \times Eligible_{ist}) + \varepsilon_{ist}
\]

where \(X\) is a vector of characteristics, including: child sex, age in the given month (estimated based on July 1 in their year of birth), insurance product type (health maintenance organization, point of service, preferred provider organization, exclusive provider organization, indemnity/other), enrollment in a consumer-directed health plan (CDHP, yes/no), and calendar month.

A third model specification built off of the second one, replacing the binary indicator \(M mandate_{ist}\) with a categorical variable for the number of years after the mandate was implemented that separately distinguished the first, second, and third or more years after mandate implementation from the absence of a mandate.
It is standard practice to estimate these types of models via linear regression, even with a binary outcome variable, to facilitate interpretation of the model results. Because of the very large number of observations and computing constraints, we followed an approach that combined disproportionate stratified random sampling on the dependent variable and logistic regression,¹ which requires adding a constant to the estimated intercept. Because this approach does not guarantee that the mean of the predicted probabilities from the corrected model is equal to the proportion of the dependent variable in the overall population, we recalibrated the model’s predicted probabilities by adding a model-specific constant equal to the difference of the logits of the outcome’s overall proportion and the mean of the model’s predicted probabilities. Standard errors were adjusted to account for the clustering of observations within states.² To ease interpretation, we converted the model results to predictive margins on the probability scale and used Wald tests to compare predictive margins statistically.

Testing the DiD “Parallel Trends” Assumption

To explore trends in treated prevalence in the absence of an autism mandate, we estimated an unadjusted model that produced prevalence rates for eventual mandate states in each of the three years prior to mandate implementation (combining the fourth year prior to implementation with the third year prior) as well as for never-mandate states in all years together. Unadjusted treated prevalence estimates from this model are displayed in eTable 2.

One concern might be that there is a baseline difference in treated prevalence between eligible and ineligible children in states that never implement a mandate (before the end of our study period). We failed to reject the null of equality ($P=.73$).

A second concern might that there is a difference in treated prevalence between the states that never implement a mandate and those that eventually implement a mandate in the years prior to implementation. We compared treated prevalence in each of the three pre-implementation periods with treated prevalence in the states that never implement mandates separately by child eligibility status. We failed to reject the null of equality for each pairwise comparison ($P\geq.21$).

A third concern might be that the trends in treated prevalence differ by child eligibility status during the years prior to mandate implementation. To check this, we computed the pairwise differences between time periods prior to implementation (e.g., second minus first) with an eligibility category, and then compared these differences between eligible and ineligible patients. Again, we failed to find evidence of any statistically significant difference between eligible and ineligible children in the year-to-year slopes ($P\geq.86$).
Alternative Specifications

A potential alternative strategy would have been a triple-differences specification, in which we would examine differences in treated prevalence before and after mandate implementation among eligible and ineligible children in states with and without mandates. This approach was deemed inferior because calculating the before and after difference would require assignment of an arbitrary index date during our study period for mandate implementation among states that did not implement mandates prior to the end of our study period.

As one sensitivity test, however, we estimated a DiD model among children from the subset of states that implemented a mandate during the study period (so we could observe pre- and post-implementation experience within each state). An additional advantage of this approach is that it obviates the need to make an assumption about whether children in states for which we did not observe mandate implementation would have been eligible under a hypothetical counterfactual mandate. The results from this specification were not substantively different; among eligible patients, those in mandate states had statistically significantly higher prevalence rates (P≤.001), and that difference was statistically significantly larger than the comparable difference among ineligible patients (P≤.001).

As another sensitivity test, we also modified our assumption about the age coverage among states that did not implement mandates prior to 2013. In our main analyses, we assumed that states that did not implement mandates prior to 2013 would have covered all fully insured children 0-21 years old in their counterfactual mandates. As a sensitivity test, we estimated models in which children in states without mandates would have been covered by a counterfactual mandate only if they were in fully insured plans and were 0 to 18 years old. The results from this specification were consistent; among eligible patients, those in mandate states had statistically significantly higher prevalence rates (P≤.039). Moreover, that difference was larger than the comparable difference among ineligible patients; in the adjusted specification the difference was statistically significant (P=.014), but it was not in the unadjusted specification (P=.38).

References


**Table 1.** Definition of Eligible and Ineligible Children in Mandate and Nonmandate States

<table>
<thead>
<tr>
<th></th>
<th>Mandate State</th>
<th>Nonmandate State</th>
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<tbody>
<tr>
<td><strong>Eligible child-months</strong></td>
<td>Child-months in which a child was enrolled in a fully insured plan and met mandate age criteria in a state that implemented a mandate on or before that month.</td>
<td>Child-months in which a child was enrolled in a fully insured plan and met mandate age criteria in a state that eventually implemented a mandate (during our study period).; or Child-months in which a child was enrolled in a fully insured plan and was 0-21 years old in a state that never implemented a mandate (during our study period).</td>
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<tr>
<td><strong>Ineligible child-months</strong></td>
<td>Child-months in which a child was enrolled in a self-insured plan and was 0-21 years old in a state that implemented a mandate on or before that month.</td>
<td>Child-months in which a child was enrolled in a self-insured plan and did not meet mandate age criteria in a state that eventually implemented a mandate (during our study period).; or Child-months in which a child was enrolled in a self-insured plan and was 0-21 years old in states that eventually implemented a mandate (during our study period); or Child-months in which a child was enrolled in a self-insured plan and was 0-21 years old in a state that never implemented a mandate (during our study period).</td>
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**eTable 2. Treated Prevalence**

<table>
<thead>
<tr>
<th></th>
<th>Years Before Mandate Implementation</th>
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<tbody>
<tr>
<td></td>
<td>Never-Mandate</td>
</tr>
<tr>
<td>Eligible Children</td>
<td>1.5</td>
</tr>
<tr>
<td>Ineligible Children</td>
<td>1.6</td>
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</table>