

# How State Surprise Billing Protections Increased ED Visits, 2007-2018: Potential Implications for the No Surprises Act

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For several decades, many hospital patients have faced surprise bills because they have unknowingly used out-of-network providers within in-network hospitals. The insurer covers an “allowed amount,” where the insurer may, for example, pay 70% of that amount, whereas the patient pays the other 30% via a co-payment. However, this allowed amount is often less than what the out-of-network provider charges. The patient must pay the remaining balance of this charge, unless the patient is protected by a state balance billing ban. Often this balance comes as a surprise to patients who did not know that they had used an out-of-network physician at an in-network hospital.

Although 33 states as of 2020 had banned such balance billing,<sup>1</sup> under the Employee Retirement Income Security Act (ERISA) of 1974, these state laws do not apply to the millions of Americans with employer-sponsored self-insured plans. Thus, surprise bills were still quite common. For instance, in Texas in 2013, for Humana and UnitedHealthcare patients, 56% and 45%, respectively, of their in-network hospitals had no in-network emergency department (ED) physicians.<sup>2</sup> By 2017, 38% of employer-sponsored ED visits in Texas had some out-of-network billing, compared with 18% nationally. In general, in 2018, for those with unexpected bills, the median balance bill was under \$500, but 13% owed more than \$2000.<sup>3</sup> In 2020, fewer than 40% of Americans said they could afford a \$1000 surprise bill.<sup>4</sup>

To extend protections to all states and to workers and their families with self-insured plans, the federal No Surprises Act (NSA) was passed within the Consolidated Appropriations Act of 2021 (Pub L No. 116-260).<sup>5</sup> The NSA prohibits ED balance billing everywhere in the United States (including air ambulance bills but not ground ambulance bills).

Recent work has examined how the NSA might affect ED prices. Like most state balance billing laws, the NSA has provisions to resolve out-of-network prices. The act requires an independent arbitration process to determine out-of-network prices, taking into account the median in-network price. The Congressional Budget Office has thus assumed that the act will cause in-network and out-of-network prices to fall to the median in-network price.<sup>6</sup>

## ABSTRACT

**OBJECTIVES:** The No Surprises Act took effect in 2022 and prevents patients from receiving unexpected emergency department (ED) out-of-network physician bills from in-network hospitals and restricts out-of-network co-payments to in-network co-payment levels. By studying similar state bans, we examine whether the large reduction in out-of-pocket payments under bans will have an unintended consequence of an increase in ED visits and spending.

**STUDY DESIGN:** We examine 16 million nonelderly, fully funded, privately insured health maintenance organization (HMO) enrollees between 2007 and 2018 from 15 states with balance billing bans for HMO ED visits and 16 states without bans as the control group.

**METHODS:** Using MarketScan data, we conduct an event study analysis and a difference-in-difference analysis of the impact of state balance billing bans on the probability of an ED visit. We use a 2-part expenditure model to estimate the impact on spending.

**RESULTS:** By analyzing 15 state-level bans, we find that the bans reduced spending per visit by 14% but spurred a demand response, an increase of 3 percentage points in ED visits, which wiped away the cost savings. Based on an ED severity index, these extra ED visits were 9% less urgent than prior to the bans.

**CONCLUSIONS:** We predict that the federal ban will result in \$5.1 billion in savings but 3.5 million more ED visits at \$4.2 billion in extra spending per year, largely negating expected savings. Health plans must be prepared to manage this spike in ED visits as the No Surprises Act takes effect.

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## TAKEAWAY POINTS

- ▶ The No Surprises Act of 2022 prevents patients from receiving surprise emergency department (ED) physician bills from in-network hospitals.
- ▶ Self-insured plans did not respond to state bans on balance billing, highlighting the potential benefit of the federal ban.
- ▶ However, we show that the act will also have an unanticipated inflationary effect on quantity: The drop in ED out-of-pocket payments under similar bans in 15 states increased ED visits to such an extent that there were no net cost savings.
- ▶ Managed care plans must prepare to coordinate care to better manage this increase in lower-severity ED visits.

However, there are also concerns that the act may be inflationary with respect to spending.<sup>7</sup> Adler found increases in negotiated out-of-network prices after a ban in New York.<sup>8</sup> For in-network prices, it is possible that providers will act strategically after the ban to ratchet up the median in-network price to influence the out-of-network arbitration process. Anecdotal reports from physicians under the New York ban indicate that in-network prices increased,<sup>9</sup> which contrasts with evidence from at least 1 New York insurer plan in which there was a drop in in-network prices.<sup>10</sup> So, the inflationary effect of the NSA on prices is still an unsettled question.

In this article, we instead posit that there may be a much larger, unanticipated inflationary quantity effect of the NSA in terms of increased ED utilization. Indeed, it is well known, especially from the RAND Health Insurance Experiment, that decreasing the out-of-pocket payment for ED visits increases the number of ED visits among the commercially insured.<sup>11,12</sup> With the elimination of balance billing and a decrease in allowed out-of-pocket ED amounts, estimated to be as large as \$621 per visit by Biener et al, one would expect ED demand to increase significantly under state bans, especially because a majority of Americans have been worried about affording surprise bills.<sup>13</sup> In 2018, two-thirds said they were either “very worried” (38%) or “somewhat worried” (29%) about being able to afford unexpected medical bills. One in 10 had a surprise out-of-network bill.<sup>14</sup> When states pass bans, they usually receive enormous national and local media coverage. Moreover, plans are required to disclose the new plan changes to the policyholders, particularly about no longer being held liable for balance bills. Thus informed, many individuals are likely to use the ED more often after a state ban. No research has yet estimated this demand effect. In this study, we estimate this effect across 15 state balance billing bans from 2007 to 2018. We focus on commercial health maintenance organization (HMO) ED visits because the earliest wave of state balance bill bans applied only to HMO ED visits. We then discuss demand implications for the 2022 NSA.

## METHODS

## Data and Study Population

Much of the recent surprise billing literature on out-of-network visits uses the IBM MarketScan Commercial Claims and Encounters

Database from IBM Watson Health.<sup>3,15-17</sup> The database assembles insurance claims for approximately 100 medium-sized and large employers and reports actual transaction prices paid by patients and insurers. We extracted all ED claims of nonelderly individuals enrolled in fully insured HMOs from the MarketScan database during 2007 to 2018. Fully insured plans are health plans from which the employees purchase coverage by paying premiums to the plan, and they are covered by state bans. We examine states with at least 5000 ED visits in

MarketScan. This gives 16.3 million enrollee-years with 3.8 million ED visits from 31 states: 15 states with balance billing bans for HMO ED visits at some time during the period and 16 states (including the District of Columbia) without bans as a control group (see the [eAppendix](#) [available at [ajmc.com](#)] for states). Seven of the 15 states with bans use an independent dispute resolution method.<sup>18</sup> Four of the states use price benchmarks.<sup>19</sup> We identified state bans using several sources.<sup>15,19-21</sup>

## Statistical Analysis

Our goal is to measure the impact of state balance billing bans on (1) the probability of using the ED, (2) the probability of being admitted to the inpatient unit from the ED, (3) annual (allowed) ED spending per person, and (4) the Emergency Severity Index (ESI) of the ED visits. The ESI ranges from 1 (most urgent) to 5 (least urgent) and is a code commonly used by hospitals to triage ED cases, as recommended by the Agency for Healthcare Research and Quality (AHRQ).<sup>22</sup> We use the algorithm of Wiler et al to estimate the ESI in our data.<sup>23</sup> We also use the AHRQ Clinical Classifications Software to identify common groups of diagnoses under the ESI severity levels.<sup>24</sup>

We use a difference-in-differences methodology at the enrollee-year level (and at the ED visit level for the ESI analysis), with metropolitan statistical area (MSA)-state fixed effects and SEs clustered at the state level. This difference-in-differences method can sometimes be biased when treatment effects are heterogeneous.<sup>25</sup> We check this using the *twowayfweights* Stata program of de Chaisemartin et al and verify that our difference-in-differences estimates are not biased because of this issue ([eAppendix Table 1](#)).<sup>26</sup> Nevertheless, to deal with any potential biases, as suggested by Goodman-Bacon, we further examine the dynamics of the bans using the event study methodology.<sup>27</sup> To do this, we focus on the states where we can observe trends both several years before and after the passing of the bans, along with the 16 states without bans. This event study subsample of 21 states consists of 11 million HMO enrollee-years and includes 5 states with bans: California, Connecticut, Illinois, Massachusetts, and Texas. This is a mix of states with differing mechanisms to determine out-of-network prices: regulated prices (benchmarks), independent dispute resolution, hybrid methods, and no prescribed methods.

We also estimate the impact of state bans on annual ED spending per enrollee in the event study subsample using a 2-part expenditure estimator. Finally, as a placebo test, we repeat all our regressions for self-insured HMO plans, which are self-insured by the employer and are exempt from state regulations under ERISA. These plans should not be affected by state balance billing bans (this is confirmed in [eAppendix Tables 3 and 5](#)). As a second placebo test, we also run our regressions on Medicare fee-for-service (FFS) ED claims. Medicare has prohibited balance billing since 1997. See the [eAppendix](#) for all method details.

## RESULTS

### ED Visits

In [Table 1](#), we see that the adoption of 15 state balance billing bans for HMO ED visits was associated with an increase in the probability of an ED visit by 2.6 percentage points ( $P < .05$ ). However, this increase in ED visits had no impact on inpatient admissions from the ED ([Table 1](#)). Also, the 15 bans were associated with an increase of 0.27 in the ESI ( $P < .01$ ), from 3.09 to 3.36 (a 9% increase). Recall that high ESI means low severity. Thus, this result indicates that these extra ED visits due to the ban were 9% less urgent than before the ban.

As a robustness check, we focus on the 5 states that passed bans during 2009 to 2017 so we can analyze what happened before and after the bans compared with the 16 control states without bans. The event study in the [Figure](#) shows an increase of approximately 3 percentage points (29% increase) in the probability of an ED visit after the bans, and this effect also remained stable at about 3 percentage points at least 5 years out ( $P < .01$ ).

Our results may possibly be driven by the reforms of the Affordable Care Act (ACA) during the latter part of our period rather than the balance billing laws. Although the ACA did not prohibit balance billing, it did impose other provisions that may have affected ED visits by altering ED demand and cost structures.<sup>28</sup> As a placebo test, we examine the impact of bans on self-insured plans. These plans are exempt from state bans but not from the ACA. We find in [eAppendix Table 3](#) that the 15 state bans did not affect the probability of an ED visit in self-insured plans. In [eAppendix Table 5](#), we see that the state bans had no impact on annual out-of-pocket ED spending in self-insured plans. Thus, our results for fully insured plans are not likely driven by ACA reforms

**TABLE 1.** Estimates of the Effect of 15 Balance Billing Bans on Fully Insured HMO ED Visits, 2007–2018<sup>a</sup>

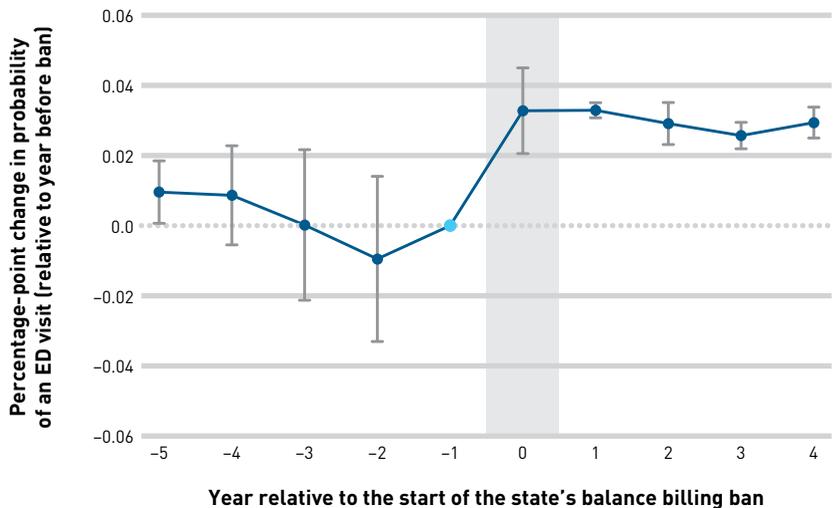
	Estimated percentage-point change due to bans	Percent effect
Probability of an ED visit during the year	2.6** (1.0)	20.3%**
Probability of an inpatient admission via an ED visit during the year	0.2 (0.2)	2.0%
Emergency Severity Index (1, most urgent; 5, least urgent)	0.27*** (0.06)	9.0%***
N	16 million	

ED, emergency department; HMO, health maintenance organization; MSA, metropolitan statistical area. \*\* $P < .05$ ; \*\*\* $P < .01$ .

<sup>a</sup>Rows 1–2: 16 million enrollee-years. Row 3: 3.8 million ED visits. Observations are across 31 states, 15 of which had balance billing bans in place between 2007 and 2018. Estimates of the impact of bans control for age, sex, 27 comorbidities, MSA income, physician supply, year, and MSA-state fixed effects ([eAppendix Table 3](#)). Robust SEs clustered at the state level are reported in parentheses.

Source: Authors' calculations using MarketScan data.

**FIGURE.** Event Study of the Effect of 5 State Balance Billing Bans on the Probability of an HMO ED Visit: 2007–2018<sup>a</sup>



ED, emergency department; HMO, health maintenance organization; MSA, metropolitan statistical area. <sup>a</sup>N = 11 million nonelderly privately and fully insured HMO enrollee-years across 21 states, 5 of which had balance billing bans sometime between 2009 and 2017. We report 95% confidence bands, adjusted for clustering at the state. Time 0 is the year of the ban, with year -1 omitted as reference group, also containing states with no ban during the period. Regression controls for age, sex, 27 comorbidities, MSA income, physician supply, year, and MSA-state fixed effects, and 18 event time dummies.

Source: Authors' calculations using MarketScan data.

but instead by actual state bans. We also find that the bans did not affect Medicare FFS ED visits ([eAppendix Figure 1](#)), as expected because Medicare has prohibited balance billing since 1997.

The parallel trends assumption for our models is shown to hold in [eAppendix Table 6](#). In [eAppendix Table 7](#), several robustness checks are demonstrated. ED visits increase with bans even after controlling for the MSA's level of physicians integrated with hospitals and for the MSA's hospital Herfindahl-Hirschman Index. Results are also robust when we leave out 115 MSA-states that had changes in the MarketScan employer composition. Results are

**TABLE 2.** Two-Part Model Estimate of the Net Effect of Balance Billing Bans on Annual ED Spending per Person, 2007-2018

Event study subsample	Probability of an ED visit	Annual ED spending conditional on use	Expected annual ED spending per capita
Ban	0.138*** (0.004)	\$1207*** (47)	\$165*** (8)
No ban	0.108*** (0.002)	\$1407*** (30)	\$152*** (4)
Difference	0.03*** (0.005)	-\$200*** (61)	\$13 (12)

ED, emergency department; HMO, health maintenance organization; MSA, metropolitan statistical area.

\*\*\* $P < .01$ .

\*N = 11 million privately and fully insured, nonelderly HMO enrollees across 21 states, 5 of which had balance billing bans starting between 2009 and 2017. Probability of an ED visit estimated by logit, and conditional spending estimated by generalized linear model with a log link and gamma distribution (eAppendix Table 4). Column 3 spending = column 1  $\times$  column 2. Spending is wage and inflation adjusted to 2018 US\$, and risk adjusted for age, sex, 27 comorbidities, MSA income, physician supply, year, and MSA-state fixed effects. Allowed prices shown; balanced bill not included. Robust SEs clustered at the state level are reported in parentheses.

Source: Authors' calculations using MarketScan data.

robust to potential outlier states using the leave-one-out method, as seen in eAppendix Table 8.<sup>29</sup> In eAppendix Table 9, we show that most of the demand effect of bans is because of states with a dispute resolution process,<sup>30</sup> a change of 2.6 percentage points vs 1.2 for states without a dispute resolution process.

### ED Spending

Despite this increase in the probability of an ED visit after the bans, we do not observe a net increase in annual ED spending per person. In Table 2, we estimate a 2-part model for the 21-state sample. First, the bans increase the probability of an ED visit from 10.8% to 13.8% ( $P < .01$ ). Next, the bans decrease annual spending on ED visits for individuals with a visit by 14% from \$1407 to \$1207 ( $P < .01$ ). Most of this decrease is due to a \$148 decrease in annual allowed out-of-pocket spending on the ED (eAppendix Table 5). Hence, the expected spending per person under the bans is \$165 ( $= 0.138 \times \$1207$ ) compared with \$152 ( $= 0.108 \times \$1407$ ) without a ban. Thus, the net effect of the bans is an expected increase of \$13 per person (\$165 – \$152). This net effect is not statistically significant. Thus, bans have no impact on ED spending in terms of allowed spending.

Finally, although our data do not have the unallowed balance bill amount, using Medical Expenditure Panel Survey estimates of the unallowed balance billing charge from Biener et al,<sup>13</sup> we estimate the mean balance billing charge in our data to be \$122 per ED visit (eAppendix). Next, we have estimated the reduced out-of-pocket on the allowed amount to be a mean savings of \$148 per ED visit (eAppendix Table 5). Thus, the expected total reduction in out-of-pocket spending per ED visit under the bans is \$270 ( $= \$148 + \$122$ ). Thus, although there are no overall net savings to society from the bans because of the demand effect, the bans do reduce the expected out-of-pocket cost of an ED visit substantially for the patient.

## DISCUSSION

With such a large reduction in expected out-of-pocket cost of an ED visit, one would expect a possible increase in ED visits after a surprise bill ban, especially with all the media coverage that often surrounds the passage of the ban. Because individuals will no longer have the fear of a possible catastrophic surprise ED bill not covered by their insurer, they may be more inclined to go to the ED in marginal, less severe cases. We estimate an increase of 3 percentage points in ED visits under state bans, with no commensurate increase in inpatient admissions from the ED. We also find that the mean ESI, measured on a scale from 1 (most urgent) to 5 (least urgent), increased by 9% after a ban. These results suggest that this increase in ED visits is indeed composed of less severe, less urgent cases. In fact, we find that the additional ED visits are most often for the diagnosis categories of “sprains and strains” and “superficial injuries; contusions” and less often for the category “nonspecific chest pain.”

Is the size and form of this demand increase reasonable under a \$270 decrease in charges per ED visit? Selby et al found that an increase in HMO ED co-payments from \$25 to \$35 (1993 US\$) resulted in a 29% drop in the number of ED visits coded as “often not an emergency.”<sup>11</sup> Hsu et al used a natural experiment at Kaiser Permanente HMOs to examine the effect of co-payments on ED use.<sup>12</sup> They found that an increase in ED co-payments from \$0 to \$100 (measured in 2001 US\$) reduced ED visits by 23%, or 4.7 percentage points. This is larger than our result but comparable: A \$270 decrease in out-of-pocket payments leads to an increase of 3 percentage points (29%) in visits (in 2019 US\$).

Our results have implications for the 2022 NSA. We have shown that self-insured plans did not respond to state bans on balance billing, highlighting the potential substantial effect of the federal ban. Previously, some states have attempted to encourage self-insured plans to participate in state-based balance billing regulation. For example, Washington made a significant effort to recruit self-insured employers to voluntarily opt into its 2020 balance billing regulation,<sup>31</sup> and New Jersey allowed self-insured plans to opt into the state's balance billing dispute resolution process in 2018. However, these cases are not typical.<sup>32</sup> Our results indicate that the NSA will likely affect many individuals in self-insured plans, as well as those in the states currently without state bans. We estimate this to be approximately 115 million individuals, with 2.4 million a year no longer receiving a surprise out-of-network ED bill (eAppendix).

The NSA will provide these 115 million privately insured adults with substantial additional financial protections. Using Table 2, in which we see that state bans lowered expected allowed annual spending on ED visits by \$200 per person using the ED (ie, due to less out-of-network use, lower prices, and the increase in less severe cases), we estimate that the NSA ban would decrease the annual allowed ED spending by \$3.2 billion ( $115 \text{ million} \times 0.138 \times \$200$ ) a year. Adding in the \$122 in balance

billing per ED visit, the NSA would save another \$1.9 billion per year (115 million  $\times$  0.138  $\times$  \$122). Overall, the NSA will result in \$5.1 billion in savings per year (\$3.2 billion + \$1.9 billion), with a 95% CI of \$3.1 billion to \$7.1 billion. However, applying the increase of 3 percentage points in ED visits after state bans to the NSA, annual ED spending for the 115 million newly covered will increase by \$4.2 billion per year (115 million  $\times$  0.03  $\times$  \$1207), with a 95% CI of \$3.9 billion to \$4.5 billion, with 3.45 million extra ED visits per year (115 million  $\times$  0.03). Therefore, the NSA will result in overall net savings of \$900 million (\$5.1 billion – \$4.2 billion) per year ( $P = .39$ ), with a 95% CI of –\$1.1 billion to \$2.9 billion. Thus, our predicted demand response to the NSA is not inconsequential and could wipe away the potential cost savings of the NSA.

How hospitals and insurers deal with this increased ED demand will be of interest. In 2018, to stem ED visits, Anthem began a policy for the commercially insured of not covering ED visits that it deemed, retroactively, to be nonemergent.<sup>33</sup> As of July 1, 2021, UnitedHealthcare proposed to begin a similar policy for the commercially insured, although this has been delayed because of its unpopularity.<sup>34</sup> Although such retroactive denials would deny approximately 15% of ED visits, they have been shown to be ineffective at reducing the truly unnecessary ED visits.<sup>33</sup> Future research should examine better methods of detecting unnecessary ED visits, as well as the new role of urgent care facilities, retail clinics, telehealth, and stand-alone EDs in this environment. Moreover, additional future research should focus on the impact of bans on patient medical debt and bankruptcy.

### Limitations

Our study has several limitations. First, it is not nationally representative. It is biased toward large employers and does not include small employers. Many small firms with fewer than 100 employees are often not self-insured and are covered by state bans. Second, we observe only allowed spending; we cannot estimate the size of balance bills (unallowed charges) and rely on the estimate of Biener et al.<sup>13</sup> We presume that state bans eliminated these balance bills, but it is quite possible that some providers still illegally overcharge patients. Finally, our results hold for HMOs, not necessarily for preferred provider organizations (PPOs). Not all the state bans we studied applied to PPOs. Future research should examine PPO patients under the few bans that also applied to PPOs.

## CONCLUSIONS

We have highlighted an overlooked aspect of the surprise billing literature, as papers so far have failed to account for a demand response to state prohibitions on surprise bills in the ED. We have shown that these bans result in large decreases in out-of-pocket costs for the consumer, which in turn encourages more ED visits for less severe cases after the ban. Because this may also occur more broadly for the 2022 NSA, insurers and primary care physicians may want to renew efforts to prevent costly, avoidable ED visits. ■

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## Online Supplemental Material

### How State Surprise Billing Protections Increased ED Visits, 2007-2018:

#### Potential Implications for the No Surprises Act

### Technical Appendix

#### *State Bans*

Our states with bans: California, Connecticut, Illinois, Massachusetts, New Hampshire, North Carolina, Oregon, Texas, Colorado, Delaware, Florida, Indiana, New Jersey, New York, and Pennsylvania. Our control states: Washington DC, Alabama, Georgia, Idaho, Kansas, Kentucky, Louisiana, Michigan, Nevada, Ohio, Oklahoma, South Carolina, Tennessee, Utah, Virginia, and Washington.

#### *Regression Models*

In all regressions, we include 6 age categories, sex, and 27 Elixhauser comorbidities as covariates. All estimates include MSA-state and year fixed effects, with standard errors clustered at the state. Census tract data from the American Community Survey are used to control for MSA income and the supply of physicians per capita. Descriptive statistics are found in Appendix Table 2.

ED spending is inflation adjusted to 2018 dollars and then adjusted for local differences by the wage index from the CMS Cost Reports. To estimate the impact of bans on spending, we use the two-part expenditure estimator via Belotti et al.'s (2015) Stata package. The first stage is a logit estimate of any ED use during the year. A modified Park test indicates that the second stage Generalized Linear Model (GLM) estimate of ED expenditures conditional on use should follow a log link with a gamma family distribution (Deb and Norton, 2018). Each stage uses unconditional MSA-state and year fixed-effects. We then multiply the results of the two stages to estimate expected annual (allowed) savings from the balance billing bans.

### *Event Study Model*

Our event study equation is as follows:

$$y_{imt} = \alpha_m + \sum_{k=-9}^8 \beta_k * 1(t - t^* = k) + X_{imt}\gamma + \tau_t + \epsilon_{imt} \quad (1)$$

where  $y$  is the outcome of interest (e.g., an ED visit) observed for enrollee  $i$  in year  $t$  in MSA-state  $m$ ,  $\alpha_m$  is an MSA-state fixed effect,  $t^*$  is the year of the ban,  $k$  is event time relative to the year of the ban ( $k=0$  in the year of the ban), and  $X$  represents other covariates. Note that  $1(t - t^* = k)$  are event time dummies indexing duration as number of years relative to the ban, and hence independent of the year fixed effects,  $\tau$ . The main coefficients of interest are the  $\beta_k$ 's on the event time dummies. Following the literature's convention, event time  $k=-1$ , the year prior to the move, is the omitted category for the event time dummies, and  $\beta_k$  is normalized to zero for people living in states that never had bans.

### *Difference-in-Difference Model with Heterogeneous Treatment Effects Test*

We also estimate the effect of the ban  $A_{mt}=0,1$ , using a two-way fixed-effects (TWFE) model:

$$y_{imt} = \alpha_m + \beta_{fe}A_{mt} + X_{imt}\gamma + \tau_t + \epsilon_{imt} \quad (2)$$

where the MSA-state fixed effects account for potential time invariant confounders and the year fixed effects control for overall time trends associated with ED prices and use. Ignoring  $X$  for the moment, it is shown by de Chaisemartin and D'Haultfoeuille (2020) (CH) that the TWFE estimator is

$$\beta_{fe} = E \left( \frac{1}{N} \sum_{m,t:A=1} w_{mt} N_{mt} \Delta_{mt} \right), \quad (3)$$

where  $\Delta_{mt}$  is the average treatment effect in cell  $(m,t)$ ,  $N_{mt}$  is the number of observations in  $(m,t)$ , and where the derived weights are defined as

$$w_{mt} = \frac{\epsilon_{mt}}{\frac{1}{N} \sum_{m,t:A=1} [N_{mt} \epsilon_{mt}]} , \quad (4)$$

and where the residuals  $\epsilon_{mt}$  are estimated from regressing ban adoption  $A_{mt}$  on MSA-state and time fixed-effects. In general, as noted by Goodman-Bacon (2018) and CH, it might not be the case that  $E(\beta_{fe})$  equals the overall average treatment effect, especially if the treatment effects

$\Delta_{mt}$  are not constant across MSA-states and time in (3). Thus, the TWFE may be a biased estimator of the average treatment effect of bans if the effects are not homogeneous across MSA-state and time. However, in their Corollary 2, CH show that the TWFE estimator is not biased if the weights are not correlated with  $\Delta_{mt}$ . To show this, we follow their recommended test. Using CH's Stata module, *twowayfweights* (de Chaisemartin, D'Haultfoeuille, and Deeb, 2020), we compute the  $w_{mt}$ 's and run tests regressing  $w_{mt}$  on time and MSA traits possibly related to the size of the treatment effect, each for our two outcomes in Exhibit 4, the probability of an ED visits and annual ED spending. That is, in Appendix Table 1 we test if those correlations between weights and time and MSAs significantly differ from zero is a way to assess whether the weights are as good as randomly assigned to MSA-states and time periods.

eAppendix Table 1: de Chaisemartin and D'Haultfoeuille's Random Weight Test

	Probability of an ED Visit: Weights $w_{mt}$	Annual ED Spending: Weights $w_{mt}$
Year	-0.08 (0.09)	0.14 (0.19)
Log(MSA income)	-0.0005 (0.0037)	-0.005 (0.008)
Log(MSA doctors per 1,000 capita)	-0.004 (0.011)	0.015 (0.020)

Notes: 11 million private fully-insured, non-elderly HMO enrollees across 21 states, 5 of which had balance billing bans starting between 2009 and 2017. Spending is wage and inflation adjusted to 2019 dollars.

Weights are defined in equation (4). Regressions are performed using *twowayfweights*.

Source: Authors' calculations using MarketScan Data.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Since all estimated correlations in Appendix Table 1 are not significant, it is highly unlikely that the weights are heterogeneous over time and across MSAs in terms of the effects of bans. They are no better than randomly assigned to (m,t) cells. This is also corroborated in Figure 1, where the ban effects are stable over time. Finally, not shown, de Chaisemartin and D'Haultfoeuille's placebo tests hold, verifying parallel trends, as seen in Figure 1. As a result, de Chaisemartin and D'Haultfoeuille's Corollary 2 indicates that our difference-in-differences estimates are likely unbiased.

#### *Accounting for States with Secondary Bans*

We focus primarily on the states' first ban on balance billing for HMOs. Some states later introduced more comprehensive bans that extended to PPOs and/or added dispute resolution mechanisms. Our 36-state analysis just considers the first ban. Our 21-state analysis with a 5 state event study sets California observations to missing after 2014, so as to focus on the 2009 ban rather than the 2017 ban.

#### *Estimating the Balance Bill from Biener et al. (2021).*

Recently, Biener et al. (2021) used the Medical Panel Expenditure Survey to estimate the total ED physician charge on surprise out-of-network bills to be \$789 for years after 2010, of which the insurer paid \$168. To estimate the allowed portion of that out-of-pocket, for ED visits without a surprise we estimate the ratio of allowed out-of-pocket to allowed total payment:

$16.3/344.78 = .05$ . Since ACA requires the allowed out-of-network and in-network cost-sharing to be the same, we apply the 0.05 to the total allowed payment for ED visits with surprise bills:

$(168.3+x) \cdot .05 = x$ , where  $x$  is the allowed out-of-pocket. This yields  $x=8.9$ . Thus, the unallowed balance bill charge for the out-of-network portion of a visit is \$611.6 ( $=788.8-168.3-8.9$ ). We can also write this as  $611.6 = 788.8 - 168.3 - 168.3 \cdot (.05/.95) = 788.8 - 1.05 \cdot 168.3$ . Since we know the standard errors for 788.8 and 168.3, we can use a z-test to calculate the standard error of 611.6 to be 109.38. Next, the rate of out-of-network use in our data for states without bans and for the self-insured is 20%. Thus, the expected unallowed balance bill for any ED visit not under bans is \$122.3 ( $=611.6 \cdot 0.2$ ), with a standard error of \$21.9.

### *Estimates of the Impact of the No Surprises Act*

We also compute how many people in the U.S. will be newly exposed to a balance billing ban due to the federal ban in 2022. Since our results reveal that self-funded plans have not been affected by state bans, we first estimate from the 2020 KFF Employer Health Benefit Survey (2020) (based on the American Community Survey) that all 105.9 million people insured under self-funded employer plans will be newly protected by the No Surprises Act (NSA). To this, we add the 9.2 million in fully-insured employer plans in the 18 states without state bans (including Minnesota, whose ban does not apply to emergencies). This sums to 115.1 million people receiving new protections under the NSA. Compared to the 43.3 million currently protected by state bans, this is a 168% increase in the number of people protected from balance billing. Since 10.3% (under no ban) go to the ED and 20% of those generally receive surprise bills per year (from our MarketScan data), we estimate 2.4 million ( $=0.2 \cdot 10.3 \cdot 115.1\text{m}$ ) per year will be spared surprises under the NSA.

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*Regression Tables*

eAppendix Table 2: Descriptive Statistics of HMO Enrollee Sample:  
2007-2018

	16 States without Bans	15 States with Bans
Ban	0.0000 (0.0000)	0.7593 (0.0001)
ED visit	0.1259 (0.0002)	0.1278 (0.0001)
Annual ED spending	201.2698 (0.5296)	178.0278 (0.2439)
Inpatient admission	0.0113 (0.0000)	0.0097 (0.0000)
Emergency Severity Index (if there is an ED visit)	3.010 (0.001)	3.275 (0.001)
Sex, female	0.5339 (0.0002)	0.5258 (0.0001)
Age	33.0571 (0.0091)	33.4013 (0.0052)
Log(median income)	10.7990 (0.0001)	10.9117 (0.0000)
Log(docs per capita)	0.1697 (0.0001)	0.4041 (0.0001)
Member days	324.9098 (0.0444)	331.5798 (0.0230)
HIV and AIDS	0.0013 (0.0000)	0.0015 (0.0000)
Alcohol abuse	0.0046 (0.0000)	0.0053 (0.0000)
Deficiency anemias	0.0270 (0.0001)	0.0314 (0.0000)
Rheum. arthritis/collagen vasc. dis.	0.0102 (0.0001)	0.0095 (0.0000)
Blood loss anemia	0.0035 (0.0000)	0.0032 (0.0000)
Congestive heart failure	0.0044 (0.0000)	0.0036 (0.0000)
Chronic pulmonary disease	0.0523 (0.0001)	0.0548 (0.0001)
Coagulation deficiency	0.0039 (0.0000)	0.0039 (0.0000)
Depression	0.0411 (0.0001)	0.0292 (0.0000)
Diabetes w/out chronic comp.	0.0524 (0.0001)	0.0473 (0.0001)
Diabetes w/ chronic comp.	0.0133 (0.0001)	0.0137 (0.0000)
Drug abuse	0.0049 (0.0000)	0.0051 (0.0000)
Hypothyroidism	0.0366 (0.0001)	0.0333 (0.0001)
Liver disease	0.0075 (0.0000)	0.0096 (0.0000)
Lymphoma	0.0012 (0.0000)	0.0014 (0.0000)

Fluid and electrolyte disorders	0.0142 (0.0001)	0.0096 (0.0000)
Metastatic cancer	0.0020 (0.0000)	0.0021 (0.0000)
Obesity	0.0422 (0.0001)	0.0480 (0.0001)
Paralysis	0.0020 (0.0000)	0.0016 (0.0000)
Peripheral vascular disease	0.0062 (0.0000)	0.0067 (0.0000)
Psychoses	0.0179 (0.0001)	0.0222 (0.0000)
Pulmonary circulation disorders	0.0016 (0.0000)	0.0011 (0.0000)
Renal failure	0.0032 (0.0000)	0.0027 (0.0000)
Solid tumor w/out metastasis	0.0156 (0.0001)	0.0159 (0.0000)
Chronic peptic ulcer disease	0.0014 (0.0000)	0.0014 (0.0000)
Valvular disease	0.0114 (0.0001)	0.0103 (0.0000)
Weight loss	0.0066 (0.0000)	0.0050 (0.0000)
Observations	3,989,608	12,348,852

Notes: Standard errors in parentheses. Differences between balance billing states and no ban states are statistically significant at  $p < .05$  except for peptic ulcer.

eAppendix Table 3. Difference-in-Difference Estimates of the Impact of Balance Billing Bans on the Probability of an ED visit:  
Full 31 State Sample

VARIABLES	Fully-Insured Plan ED Visit	Self-Insured Plan ED Visit	Fully-Insured Plan Inpatient Admission from ED	Fully-Insured Plan Emergency Severity Index (1 most urgent, 5 least urgent)
Ban	0.026*** (0.008)	-0.001 (0.005)	0.00186 (0.002)	0.272*** (0.058)
Days enrolled during year	0.000*** (0.000)	0.0001*** (0.000)	-0.00001 (0.001)	--
Sex, female	0.010*** (0.002)	0.004*** (0.001)	-0.00252*** (0.000)	-0.037*** (0.007)
18-30 years	0.004*** (0.001)	0.005*** (0.002)	-0.00148*** (0.000)	-0.198*** (0.023)
31-40 years	-0.016*** (0.001)	-0.010*** (0.002)	-0.00287*** (0.000)	-0.365*** (0.030)
41-50 years	-0.032*** (0.002)	-0.019*** (0.002)	-0.00296*** (0.000)	-0.540*** (0.029)
50-60 years	-0.047*** (0.002)	-0.023*** (0.002)	-0.00282*** (0.000)	-0.642*** (0.030)
61-64 years	-0.059*** (0.003)	-0.028*** (0.003)	-0.00332*** (0.001)	-0.579*** (0.027)
Log(median income)	0.067 (0.104)	-0.153* (0.079)	0.00879 (0.009)	-0.480 (0.420)
Log(docs per capita)	0.010 (0.023)	-0.021 (0.042)	0.00674*** (0.002)	-0.105 (0.112)
HIV and AIDS	0.059*** (0.005)	0.041*** (0.004)	0.01935*** (0.002)	0.021 (0.028)
Alcohol abuse	0.268*** (0.010)	0.178*** (0.010)	0.10145*** (0.005)	-0.259*** (0.034)
Deficiency anemias	0.064*** (0.006)	0.048*** (0.004)	0.03040*** (0.003)	-0.175*** (0.016)
Rheum. arthritis/collagen vasc. dis.	0.060*** (0.003)	0.039*** (0.004)	0.00722*** (0.001)	-0.057** (0.024)
Blood loss anemia	0.097*** (0.011)	0.059*** (0.006)	0.04440*** (0.006)	-0.076** (0.035)
Congestive heart failure	0.137*** (0.011)	0.109*** (0.012)	0.15392*** (0.010)	-0.242*** (0.014)
Chronic pulmonary disease	0.148*** (0.006)	0.085*** (0.007)	0.02245*** (0.001)	-0.129*** (0.011)
Coagulation deficiency	0.101*** (0.004)	0.088*** (0.007)	0.09342*** (0.005)	-0.172*** (0.012)
Depression	0.094*** (0.005)	0.069*** (0.004)	0.03289*** (0.002)	-0.148*** (0.013)
Diabetes w/out chronic comp.	0.063*** (0.005)	0.039*** (0.003)	0.00987*** (0.001)	-0.118*** (0.016)
Diabetes w/ chronic comp.	0.032*** (0.004)	0.034*** (0.006)	0.02575*** (0.002)	-0.022 (0.018)

Drug abuse	0.217*** (0.008)	0.162*** (0.007)	0.10074*** (0.005)	-0.098*** (0.017)
Hypothyroidism	0.011*** (0.003)	0.011*** (0.001)	-0.00169*** (0.001)	-0.088*** (0.015)
Liver disease	0.088*** (0.007)	0.067*** (0.006)	0.03487*** (0.002)	-0.268*** (0.029)
Lymphoma	0.045*** (0.003)	0.041*** (0.003)	0.03153*** (0.003)	-0.157*** (0.056)
Fluid and electrolyte disorders	0.305*** (0.013)	0.247*** (0.016)	0.20095*** (0.014)	-0.547*** (0.022)
Metastatic cancer	0.111*** (0.008)	0.100*** (0.007)	0.10710*** (0.011)	-0.274*** (0.021)
Obesity	0.069*** (0.003)	0.050*** (0.005)	0.01923*** (0.002)	-0.144*** (0.023)
Paralysis	0.198*** (0.007)	0.146*** (0.011)	0.15703*** (0.009)	-0.358*** (0.029)
Peripheral vascular disease	0.061*** (0.006)	0.075*** (0.007)	0.04312*** (0.005)	-0.135*** (0.016)
Psychoses	0.081*** (0.006)	0.058*** (0.006)	0.03589*** (0.004)	-0.144*** (0.020)
Pulmonary circulation disorders	0.092*** (0.012)	0.110*** (0.010)	0.11946*** (0.010)	-0.194*** (0.020)
Renal failure	0.063*** (0.006)	0.066*** (0.006)	0.06644*** (0.005)	0.000 (0.019)
Solid tumor w/out metastasis	0.020*** (0.002)	0.015*** (0.002)	0.01193*** (0.001)	-0.115*** (0.011)
Chronic peptic ulcer disease	0.160*** (0.012)	0.119*** (0.009)	0.07654*** (0.005)	-0.243*** (0.016)
Valvular disease	0.088*** (0.008)	0.066*** (0.008)	0.03613*** (0.004)	-0.341*** (0.018)
Weight loss	0.077*** (0.005)	0.063*** (0.004)	0.05456*** (0.003)	-0.143*** (0.012)
Constant	-0.697 (1.124)	1.683** (0.856)	-0.08897 (0.102)	9.252** (4.547)
Observations	16,299,004	26,807,762	16,299,004	3,821,525
Number of MSA-states	410	593	410	376

Year and MSA-state fixed effects not shown. Robust standard errors clusters at the state shown in parentheses.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

eAppendix Table 4: Total Expenditure Two-Part Model  
Regressions: 21 State Subsample

VARIABLES	Probability of an ED Visit (Logit)	Annual ED Spending Conditional on Spending>0 (GLM)
Ban	0.296*** (0.061)	-0.142** (0.059)
Days enrolled during year	0.003*** (0.000)	0.000*** (0.000)
Sex, female	0.085*** (0.008)	0.017*** (0.004)
18-30 years	0.035** (0.016)	0.245*** (0.031)
31-40 years	-0.159*** (0.014)	0.237*** (0.033)
41-50 years	-0.316*** (0.020)	0.227*** (0.038)
50-60 years	-0.474*** (0.018)	0.179*** (0.037)
61-64 years	-0.584*** (0.023)	0.118*** (0.038)
Log(median income)	-0.339 (0.535)	0.776 (0.517)
Log(docs per capita)	-0.951 (1.120)	0.182 (0.606)
HIV and AIDS	0.550*** (0.059)	0.005 (0.035)
Alcohol abuse	1.669*** (0.051)	0.163*** (0.012)
Deficiency anemias	0.511*** (0.028)	0.107*** (0.009)
Rheum. arthritis/collagen vasc. dis.	0.471*** (0.017)	0.088*** (0.013)
Blood loss anemia	0.514*** (0.050)	0.015 (0.016)
Congestive heart failure	0.880*** (0.075)	0.087*** (0.021)
Chronic pulmonary disease	1.035*** (0.045)	0.169*** (0.009)
Coagulation deficiency	0.672*** (0.044)	0.076*** (0.017)
Depression	0.691*** (0.019)	0.194*** (0.010)
Diabetes w/out chronic comp.	0.605*** (0.052)	0.079*** (0.009)

Diabetes w/ chronic comp.	0.170*** (0.039)	0.025** (0.010)
Drug abuse	1.311*** (0.039)	0.378*** (0.013)
Hypothyroidism	0.159*** (0.029)	0.043*** (0.011)
Liver disease	0.695*** (0.022)	0.191*** (0.016)
Lymphoma	0.356*** (0.033)	0.078*** (0.029)
Fluid and electrolyte disorders	1.759*** (0.049)	0.262*** (0.011)
Metastatic cancer	0.691*** (0.046)	0.040** (0.017)
Obesity	0.509*** (0.022)	0.104*** (0.012)
Paralysis	1.256*** (0.051)	0.200*** (0.028)
Peripheral vascular disease	0.546*** (0.031)	0.089*** (0.019)
Psychoses	0.556*** (0.029)	0.161*** (0.018)
Pulmonary circulation disorders	0.466*** (0.067)	0.076*** (0.021)
Renal failure	0.366*** (0.049)	-0.038** (0.015)
Solid tumor w/out metastasis	0.208*** (0.027)	0.035*** (0.006)
Chronic peptic ulcer disease	1.184*** (0.050)	0.243*** (0.018)
Valvular disease	0.714*** (0.042)	0.115*** (0.019)
Weight loss	0.537*** (0.031)	0.129*** (0.008)
Constant	0.671 (5.732)	-1.698 (5.583)
Observations	10,866,697	10,866,697

Notes: year and MSA-state fixed effects not shown. Conditional spending estimated by GLM with a log link and gamma distribution. Robust standard errors clustered at the state shown in parentheses.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

eAppendix Table 5: Annual Out-of-Pocket ED Spending  
Regressions: 21 State Subsample

	Fully-Insured Plan Annual Out-of- Pocket given an ED Visit	Self-Insured Plan Annual Out-of- Pocket given an ED Visit
Ban	-147.57*** (41.98)	42.02 (44.17)
Days enrolled during year	0.08 (0.12)	-0.06** (0.02)
Sex, female	3.86 (7.16)	8.73*** (2.17)
18-30 years	88.06** (31.90)	9.15 (8.15)
31-40 years	105.60** (39.09)	10.90 (6.64)
41-50 years	105.86** (44.36)	0.06 (5.46)
50-60 years	83.67** (38.47)	-12.47** (5.31)
61-64 years	69.85 (40.69)	-20.87** (8.44)
Log(median income)	792.96 (508.14)	-12.20 (122.45)
Log(docs per capita)	-1,367.51*** (299.01)	-130.24 (110.72)
HIV and AIDS	-65.60 (66.92)	49.53*** (13.95)
Alcohol abuse	48.02 (33.96)	1.91 (1.74)
Deficiency anemias	42.92 (28.71)	23.95* (11.61)
Rheum. arthritis/collagen vasc. dis.	20.80 (13.85)	30.41* (17.36)
Blood loss anemia	-36.37* (20.63)	-9.96 (6.09)
Congestive heart failure	35.47 (29.09)	39.31** (16.24)
Chronic pulmonary disease	53.60** (25.12)	18.75*** (6.51)
Coagulation deficiency	-0.26 (18.81)	15.22 (9.83)
Depression	56.79** (24.37)	31.63** (11.77)
Diabetes w/out chronic comp.	33.28 (19.70)	24.77*** (8.64)
Diabetes w/ chronic comp.	22.50 (18.68)	-3.36 (7.99)
Drug abuse	172.73*** (57.09)	85.23*** (14.05)
Hypothyroidism	16.93 (12.43)	-3.80 (4.20)

Liver disease	70.22*** (14.26)	32.70 (21.46)
Lymphoma	14.15 (12.40)	-6.62 (13.63)
Fluid and electrolyte disorders	99.38* (50.64)	64.87** (24.86)
Metastatic cancer	-41.12 (46.68)	-15.26 (14.29)
Obesity	32.04 (25.93)	-9.64 (10.35)
Paralysis	81.73 (50.09)	42.64 (28.97)
Peripheral vascular disease	23.39 (21.67)	10.54 (9.09)
Psychoses	72.83*** (23.37)	25.07*** (2.40)
Pulmonary circulation disorders	21.66 (40.50)	29.94 (18.80)
Renal failure	94.25*** (20.02)	88.49*** (18.95)
Solid tumor w/out metastasis	-19.48 (16.38)	4.24 (14.06)
Chronic peptic ulcer disease	89.97** (33.07)	51.03 (31.05)
Valvular disease	-4.68 (17.59)	15.62 (15.67)
Weight loss	27.42 (31.89)	18.30** (8.54)
Constant	-8,055.82 (5,531.55)	360.93 (1,310.38)
Observations	1,277,321	1,521,289
Number of MSA-states	244	272

Notes: year and MSA-state fixed effects not shown. Robust standard errors in parentheses clustered at state.

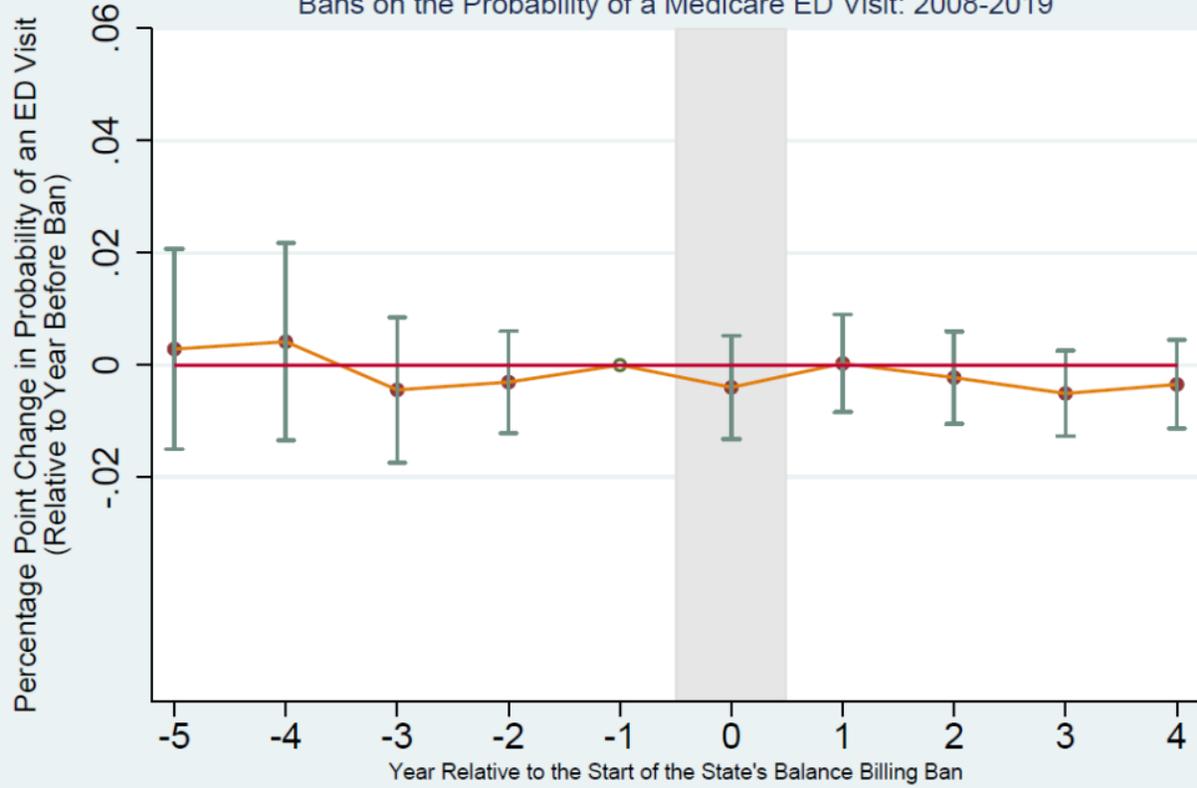
\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

eAppendix Table 6: Parallel trends test for estimates of the probability of an ED visit during the year

	Fully-insured employer, 21 state sample	Fully-insured employer, 31 state sample	Placebo: Medicare, 31state sample
<hr/>			
Leads/lags			
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Ban(t-7)	-0.0072 (0.0040)	-0.0002 (0.0086)	0.0004 (0.0104)
Ban(t-6)	-0.0035 (0.0037)	0.0031 (0.0043)	0.0072 (0.0093)
Ban(t-5)	-0.0100 (0.0073)	-0.0034 (0.0075)	0.0028 (0.0091)
Ban(t-4)	-0.0059 (0.0099)	-0.0074 (0.0110)	0.0042 (0.0090)
Ban(t-3)	-0.0103 (0.0134)	-0.0069 (0.0103)	-0.0044 (0.0066)
Ban(t-2)	-0.0184 (0.0128)	-0.0138 (0.0084)	-0.0030 (0.0046)
Ban(t)	0.0260*** (0.0041)	0.0296*** (0.0053)	-0.0040 (0.0047)
Ban(t+1)	0.0288*** (0.0029)	0.0293*** (0.0024)	0.0003 (0.0044)
Ban(t+2)	0.0188*** (0.0043)	0.0219*** (0.0047)	-0.0022 (0.0042)
Ban(t+3)	0.0285*** (0.0049)	0.0315*** (0.0058)	-0.0051 (0.0039)
Ban(t+4)	0.0396*** (0.0041)	0.0398*** (0.0043)	-0.0035 (0.0040)
Ban(t+5)	0.0214 (0.0187)	0.0288*** (0.0093)	-0.0025 (0.0041)
Ban(t+6)	0.0449*** (0.0033)	0.0386*** (0.0053)	0.0004 (0.0104)
Observations	10,868,241	16,296,930	31,896,551

Notes: t is the year the ban passed. t-1 is the omitted event time including states without bans. The pre-trends are not significantly different than those for the states without bans. Robust standard errors in parentheses.  
Test of difference compared to t-1: \*\*\* p<0.01, \*\* p<0.05

Appendix Figure 1: Event Study of the Effect of 15 State Balance Billing Bans on the Probability of a Medicare ED Visit: 2008-2019



Notes: N=32 million Medicare FFS beneficiary-years across 31 states, 15 of which had balance billing bans some time between 2008 and 2019. 95% confidence bands reported, adjusted for clustering at the state. Time 0 is year of the ban, with year -1 omitted as reference group, also containing states with no ban during the period. Regression controls for age, sex, race, year and county fixed effects, and 18 event time dummies. Source: Authors' calculations using the 5% Medicare Claims Data.

eAppendix Table 7: Estimated Probability of Using the ED During the Year:  
Additional Specifications and Robustness Checks

	All MSAs	MSAs with plans spanning all 12 years	All MSAs, controlling for hospital HHI	All MSAs, controlling for hospital-doc integration level in MSA
State ban	0.0256** (0.0103)	0.0255** (0.0104)	0.0287*** (0.0085)	0.0255** (0.0097)
Hospital HHI			0.0986* (0.0501)	
Vertical integration				-0.0367 (0.0301)
Observations	16,296,672	15,057,275	13,291,646	13,992,142
Number of MSA- States	401	286	359	397

This reruns the regression of Appendix Table 3 column 1 with various robustness checks. HHI=Herfindahl-Hirschman Index for MSA. Robust standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

eAppendix Table 8. Outlier Test: Leave-One-Out Estimates of the Probability of Using the ED During the Year

	State Left Out				
	None	CA	NY	TX	FL
State Ban	0.0256** (0.0103)	0.0254** (0.0095)	0.0266*** (0.0088)	0.0391*** (0.0033)	0.0248** (0.0100)
Observations	16,296,672	12,523,330	14,044,219	14,646,325	14,640,219

This reruns the regression of Appendix Table 3 column 1, leaving out one of the 4 largest states in the sample one at a time. Robust standard errors in parentheses.  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

eAppendix Table 9: Estimates of the Probability of Using the ED During the Year, by Ban Type

VARIABLES	Bans with Independent Dispute Resolution	All Other Bans
Ban	2.6** (1.1)	1.2*** (0.4)
Observations	14,984,480	5,295,016

Estimated percentage point change due to bans. This reruns the regression of Appendix Table 3 column 1 by type of ban. Dispute resolution states: CA, CO, DE, FL, IL, NH, NJ, NY, TX, WA. States with other types of bans: CT, IN, MA, NC, and PA, based on Maanasa Kona, “State Balance-Billing Protections,” map, Commonwealth Fund, Feb. 5, 2021. <https://doi.org/10.26099/0x7j-7731>. Robust standard errors clustered at MSA-state are in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1