Methodological Appendix[1]

This appendix provides additional detail on the methods I use to estimate how filling the Medicaid coverage gap would affect hospital finances. I begin by describing the data sources used in this analysis. Next, I lay out the difference-in-differences strategy I use to estimate the causal effect of Medicaid expansion on various relevant outcomes—including uncompensated care, insurance coverage, and hospital utilization—and present the results from that analysis. Finally, I provide detail beyond what is in the main text on how I use those estimates to estimate the effect of filling the coverage gap.

1 Data

The analyses presented here and in the main text use data from three main sources.

1.1 Insurance coverage

I obtain information on insurance coverage by state for years 2011-2019 from the American Community Survey (ACS). For each state, I derive estimates of the shares of the under-65 population that have no coverage, Medicaid coverage, direct purchase coverage, and employer-sponsored coverage. I assign people who report multiple sources of coverage a single primary source of coverage according to a hierarchy defined by the State Health Access Data Assistance Center (2020).

1.2 Uncompensated care

I obtain data on uncompensated care costs by state for calendar years 2011-2019 from hospitals' Medicare cost reports, the main data source used in prior studies of how Medicaid expansion affected uncompensated care (Dranove, Garthwaite, and Ody 2016; 2017; Rhodes et al. 2020; Moghtaderi et al. 2020).[2] Like those prior studies, I use information reported on the current CMS 2552-2010 form. This form was phased in starting during 2010, and virtually all expenses associated with calendar years 2011 and later were reported using this form rather than the older CMS 2552-1996 form.[3]

Most hospitals file cost reports that reflect fiscal years that do not align with the calendar year. To derive calendar year estimates, I allocate amounts across calendar years according to the share of the hospital's fiscal year that falls in each calendar year. One downside of this approach is that the calendar year 2013 estimates will incorporate some experience from 2014, but any resulting bias should be slight.[4] I limit my sample to acute care hospitals, which I identify based on their provider numbers.

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[1] The rest of the analysis that this appendix is a part of appears at https://www.brookings.edu/essay/how-would-filling-the-medicaid-coverage-gap-affect-hospital-finances/.

[2] I use code written by Adam Sacarny to import the raw data files published by CMS. This code is available for download at https://github.com/asacarny/hospital-cost-reports/.

[3] Specifically, when using the methodology for allocating amounts to calendar years described below, I estimate that amounts reported on the CMS 2552-1996 form account for 99.5% of reported hospital expenses incurred during calendar year 2011 and all reported expenses incurred in later years.

[4] Around two-fifths of hospitals report on a calendar year basis, and even for a hospital with a fiscal year that begins on July 1, only one-quarter of the calendar year 2013 average would reflect experience during calendar year 2014. Moreover, because Medicaid expansion enrollment ramped up gradually and because there is some lag between when care is provided and when it is determined to be charity care or bad debt, expansion likely had only limited effects on reported uncompensated care in early 2014. Consistent with this, the estimates plotted in Figure

The main outcome variable is total uncompensated care cost (Worksheet S-10, line 30). This field encompasses the cost of both charity care (care for which the hospital partially or fully waives the patient's payment obligation) and bad debt (care for which the hospital is unable to collect what it believes the patient owes). For some supplemental analyses, I also use data on charity care cost disaggregated by whether the patient is uninsured or insured (Worksheet S-10, line 23, columns 1 and 2), as well as the data on bad debt cost alone (Worksheet S-10, line 29). Finally, I extract data on hospital operating expenses (Worksheet G-3, line 4) and hospital patient revenue (Worksheet G-3, line 3).

Some hospitals report implausible uncompensated care costs. When tabulating uncompensated care costs, I exclude cost reports that report negative uncompensated care cost or that report uncompensated care costs in excess of 50% of the hospital's operating expenses. However, when calculating aggregate revenue or operating expenses, I do not exclude these cost reports.

One complication with these data is that CMS changed the definitions of the Worksheet S-10 uncompensated care fields during the period I examine (CMS 2016; 2017b). Around the same time, CMS also signaled that it was likely to begin to use these fields to allocate some Medicare disproportionate share hospital (DSH) payments (CMS 2017a). CMS allowed hospitals to revise their cost reports back to fiscal year 2014, notionally to reflect the new instructions (CMS 2017c).[5]

Whether due to the definitional changes or the changes in hospitals' reporting incentives caused by the change in the DSH policy, the changed environment appears to have increased reported uncompensated care costs. The Medicaid and CHIP Payment and Access Commission (MACPAC) analyzed the cost report data before and after the revision window and found that the revisions increased reported uncompensated care in 2015 by around 30% in the aggregate (MACPAC 2019).

For the analysis that follows, it is useful to have an uncompensated care series that reflects a consistent set of definitions and reporting practices. To approximate such a series, I use estimates of uncompensated care costs by state in 2015 derived from pre- and post-revision cost reports published by MACPAC (2018; 2019). I calculate the ratio of the pre- and post-revision amounts reported for each state. I then multiply the raw uncompensated care amounts reported by each hospital for fiscal years 2014 and later by this ratio to obtain an approximation of what would have been reported under prior definitions and practices.[6]

In what follows, I treat the resulting series as accurately reflecting hospitals' uncompensated care costs under the old reporting definitions. However, the increase in reported uncompensated care costs that coincided with the CMS policy changes might have partially reflected more complete reporting of uncompensated care encouraged by the looming use of Worksheet S-10 data for DSH purposes. In that case, my estimates could understate the true amount of uncompensated care, which would likely cause me to understate how much a coverage gap program would improve hospital finances.

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1 in the main text offer no indication that the trend in uncompensated care costs in the expansion states begin to diverge from the non-expansion states prior to 2014.

[5] For a useful more detailed summary of the steps related to Worksheet S-10 taken by CMS during this period, see Hettich, Pivec, and Polston (2017) and O'Neill (2017).

[6] I apply the same proportional adjustment to subcomponents of uncompensated care cost. This approach should be approximately correct on average across the categories but may not be exactly correct on a category-by-category basis. However, plausible alternative approaches would not change my conclusions.

I note that prior work on how Medicaid expansion affected uncompensated care has generally made no adjustment at all for these reporting changes. As reported in Table A1 below, the adjustment modestly reduces difference-in-differences estimates of how much Medicaid expansion reduced uncompensated care costs as a share of hospital expenses since it has a somewhat larger effect on non-expansion states than expansion states. The adjusted data also indicate that uncompensated care declined in non-expansion states after 2013, whereas the unadjusted data show no such decline. This may help to explain why prior work often reached the surprising conclusion that uncompensated care did not decline in Medicaid non-expansion states (e.g., Blavin and Ramos 2021; Moghtaderi et al. 2020).

1.3 Inpatient and emergency department utilization

I obtain data on aggregate inpatient and emergency department utilization by state and payer for years 2011-2018 from the Fast Stats portal maintained by the Healthcare Cost and Utilization Project (HCUP 2021). These HCUP estimates are constructed from state-maintained discharge databases that capture nearly the universe of hospital encounters in the relevant states. Prior work by Garthwaite et al. (2019) has also used data from the same underlying source to study the effect of Medicaid expansion on hospital utilization (although their data stopped in 2015 and were not obtained via Fast Stats).

The Fast Stats series for the payer categories of interest encompass encounters for people ages 19 to 64. I convert the aggregate tallies reported by HCUP to amounts per 1,000 state residents by dividing by estimates of the number of people ages 19 to 64 in each state and year derived from the ACS.

I limit my analysis of these series to states that report data for all years 2011-2018. I further exclude Nebraska and Vermont since the data documentation suggest that some encounters may not be accurately classified by payer in some years, as well as Arkansas since its Medicaid expansion was implemented through Marketplace plans, and it appears that those enrollees were typically classified in these data as having private rather than Medicaid coverage. After these exclusions, I have data on inpatient utilization for 40 states and data on emergency department utilization for 24 states.

2 Estimating the Causal Effect of Medicaid Expansion

2.1 Empirical specification

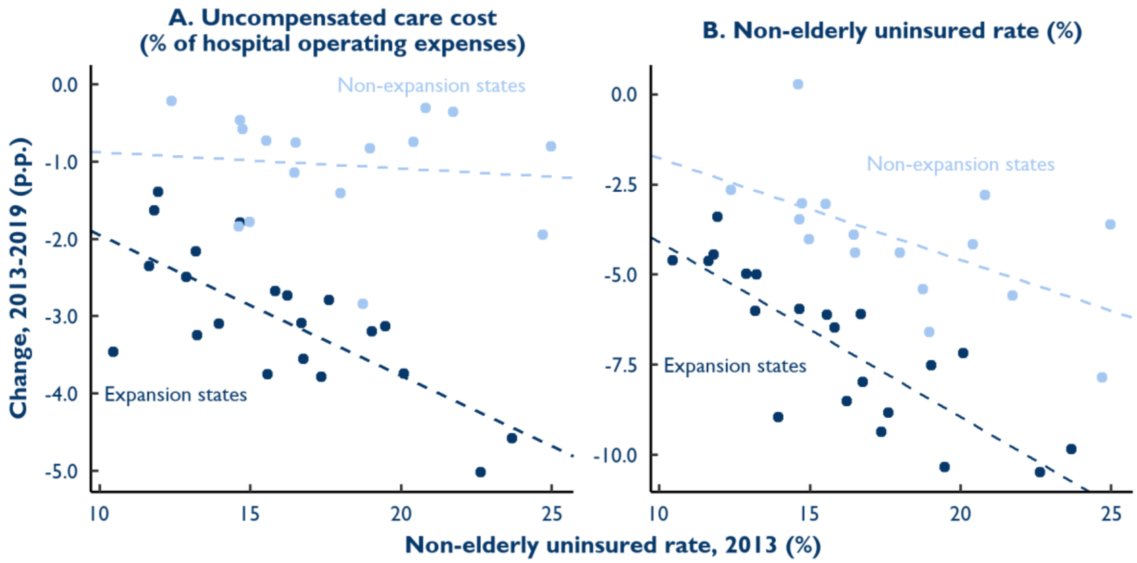
To estimate the causal effect of Medicaid expansion on the outcomes of interest, I use a difference-in-differences approach, following much of the voluminous existing literature on Medicaid expansion.[7]

In detail, I define two cohorts of states: a treatment cohort and a comparison cohort. The treatment cohort consists of states that: (1) adopted Medicaid expansion before the end of 2015; (2) had a Medicaid income eligibility threshold for non-disabled adults without dependent children below 50% of the FPL in 2013, as reported by the Kaiser Family Foundation (2021); and (3) are not Massachusetts.

The first restriction ensures that all states in the treatment cohort are likely to be close to their post-expansion steady states by the end of my data, which allows me to estimate the long-run effect of expansion. The second two restrictions, which are similar to restrictions in some prior work (e.g., Simon, Soni, and Cawley 2017; Moghtaderi et al. 2020; Rhodes et al. 2020), is intended to increase the relevance of my estimates to the coverage gap states, none of which offers Medicaid coverage to any non-disabled

[7] For an overview of that literature, see Guth, Garfield, and Rudowitz (2020) and Guth and Ammula (2021).

Figure A1. Post-2013 Changes by Expansion Status and Uninsured Rate



Note: Expansion states are states that expanded prior to 2016 and had limited childless adult eligibility in 2013. Non-expansion states are states that had not expanded as of the end of 2019, excluding Wisconsin.

childless adults. After applying these restrictions, the treatment cohort consists of 21 states (of which 18 have complete inpatient utilization data and 10 have complete emergency department utilization data).

The comparison cohort consists of all states (except Wisconsin) that had not adopted expansion as of the end of 2019. Under this definition, the comparison cohort consists of 16 states (of which 12 have complete inpatient utilization data and 8 have complete emergency department utilization data). States that are not in the treatment or comparison cohorts as defined above are excluded from the analysis.

Using these definitions, I estimate two types of regressions. The first is a simple event-study specification:

$$Y_{st} = \alpha + \beta D_s + \sum_{\substack{r \in \{2011..\bar{t}\} \\ r \neq 2013}} 1\{t = r\}[\gamma_r + \tau_r D_s] + v_{st}, \tag{1}$$

where Y_{st} is the outcome of interest in state s and year t, D_s is an indicator for whether state s is in the treatment cohort, u_{st} is the error term, and $\bar{t}$ is the final sample year (2018 when the outcome is from the HCUP data and 2019 otherwise). Under the usual common trends assumption, each coefficient τ_r is the average treatment effect on the treated states in year r (Heckman, Ichimura, and Todd 1997; Abadie 2005). I estimate equation (1) by ordinary least squares and cluster standard errors by state.

While specifications like equation (1) have been the workhorses of the Medicaid expansion literature, I also consider an alternative specification. To illustrate why, Figure A1 plots post-2013 changes in two relevant outcomes as a function of a state's 2013 uninsured rate.[8] The empirical patterns depicted in the figure suggest two distinct potential problems with estimates obtained from equation (1).

[8] The relationship between post-2013 changes and measures of different areas' "exposure" to the ACA's reforms has been noted in some prior work (e.g., CEA 2016; Dranove, Garthwaite, and Ody 2016; Garthwaite et al. 2019).

First, the figure suggests that the *difference* in post-2013 changes between expansion and non-expansion states—and thus the causal effect of expansion—was larger in states that started with higher uninsured rates. This is intuitive, as it is reasonable to expect Medicaid expansion to have had a larger effect where there was more scope to increase insurance coverage. This treatment effect heterogeneity does not compromise the internal validity of equation (1). If the usual common trends assumption holds, then a difference-in-differences specification like equation (1) will still deliver a valid estimate of the average treatment effect *on the treated* (Heckman, Ichimura, and Todd 1997; Abadie 2005).

However, for this analysis, I am interested in estimating what would have occurred in the *coverage gap states* if they had expanded; that is, I am interested in estimating the average treatment effect on (a subset of) the *untreated.* The non-elderly uninsured rate in the coverage gap states averaged 21.7% in 2013 (on a population-weighted basis), considerably higher than the (unweighted) average of 15.9% in the treatment states in that year.[9] Thus, the patterns in Figure A1 imply that using equation (1) to predict the effect of expansion in the coverage gap states would underestimate the effect in these states.

Second, the figure suggests that non-expansion states that started with higher uninsured rates in 2013 experienced larger post-2013 changes in the plotted outcomes. This is also intuitive, as it is reasonable to expect the ACA's non-Medicaid coverage provisions to have had a larger effect where there was more scope to increase insurance coverage. This fact, together with the fact that non-expansion states started with higher uninsured rates in 2013 (on average), suggests that the common trends assumption required for equation (1) to estimate a valid average treatment effect may be violated in a way that would tend to bias the estimated effect of expansion toward zero. This problem is not specific to this analysis and plausibly affects most research on Medicaid expansion to some degree.

In light of these problems with equation (1), I also consider the *conditional* event-study specification:

$$Y_{st} = \alpha(u_s^{2013}) + \beta(u_s^{2013})D_s + \sum_{\substack{r\in\{2011..\bar{t}\} \\ r\neq 2013}} 1\{t=r\}[\gamma_r(u_s^{2013}) + \tau_r(u_s^{2013})D_s] + v_{st} \qquad (2)$$

where α, β, $\{\gamma_r\}$, and $\{\tau_r\}$ are flexible functions of a state's uninsured rate in 2013, u_s^{2013}.

If the usual common trends assumption holds *conditional* on the state's baseline uninsured rate u_s^{2013}, then $\tau_r(u)$ is an average treatment effect on the treated; specifically, it is the average treatment effect in year r for treated states with baseline uninsured rate u. If, additionally, the causal effect of expansion is independent of whether a state is in the treatment cohort, conditional on the baseline uninsured rate u_s^{2013}, then $\tau_r(u)$ is also an average treatment effect on the *untreated;* specifically, it is also the average treatment effect in year r for non-treated states with baseline uninsured rate u.[10]

[9] It may seem inappropriate to compare the weighted average baseline uninsured rate in the coverage gap states to the *unweighted* average baseline uninsured rate in the treatment states. However, for my ultimate analyses, the parameter of interest is the *weighted* average treatment effect on the coverage gap states, whereas equation (1) is estimated on an unweighted basis and, as such, will estimate the *unweighted* average treatment effect on the treatment states. This weighted-to-unweighted comparison is thus the appropriate one for gauging potential bias.

[10] This assumption could fail if states that do not adopt expansion are less enthusiastic about expanding insurance coverage and, for example, make it more difficult for eligible individuals to enroll. That particular type of failure of this assumption would not be a problem for this analysis, however, since my focus is coverage gap programs that would be administered by the federal government rather than the coverage gap states themselves.

Table A1: Estimated Causal Effect of Medicaid Expansion in the Coverage Gap States

Specification:	Simple Event Study [Equation (1)]		Conditional Event Study [Equation (2)]		
Outcome	Est.	SE	Est.	SE	p-value for $\{\gamma_r\}$ and $\{\tau_r\}$ constant
A. Uncompensated care outcomes (% of operating expenses)					
Uncompensated care	-2.07	0.24	-3.00	0.31	0.001
Charity care, uninsured patients	-1.42	0.21	-1.91	0.51	0.228
Charity care, insured patients	-0.31	0.15	-0.18	0.34	0.138
Bad debt	-0.34	0.18	-0.91	0.28	0.000
Memo: Uncompensated care, without reporting adjustment	-2.69	0.27	-3.75	0.42	0.002
B. Coverage outcomes (% of non-elderly population)					
Medicaid	4.62	0.51	7.23	0.59	0.000
Uninsured	-2.87	0.65	-4.69	0.88	0.000
Direct purchase	-0.63	0.35	-1.05	0.53	0.009
Employer-sponsored	-1.14	0.49	-1.39	0.63	0.618
C. Inpatient utilization (non-elderly adult visits per 1,000 people)					
Medicaid	8.9	1.2	14.3	1.5	0.000
Uninsured	-5.2	0.8	-9.4	0.8	0.000
Private	-2.7	0.6	-2.6	0.9	0.002
Medicaid + Uninsured + Private	1.0	1.3	2.3	1.7	0.082
D. Emergency department utilization (non-elderly adult visits per 1,000 people)					
Medicaid	61.8	13.4	93.8	17.1	0.088
Uninsured	-37.9	10.7	-54.4	17.1	0.505
Private	-16.7	6.6	-19.9	4.5	0.008
Medicaid + Uninsured + Private	7.2	11.2	19.5	9.6	0.514

It is, of course, likely that these conditional common trends and treatment effect homogeneity assumptions may not hold exactly (although the amount of variation explained by the baseline uninsured rate suggests that they may be reasonable approximations). Regardless, Figure A1 makes clear that these conditional common trends assumptions are likely improvements over the *unconditional* common trends and treatment effect homogeneity assumptions required to justify use of equation (1).

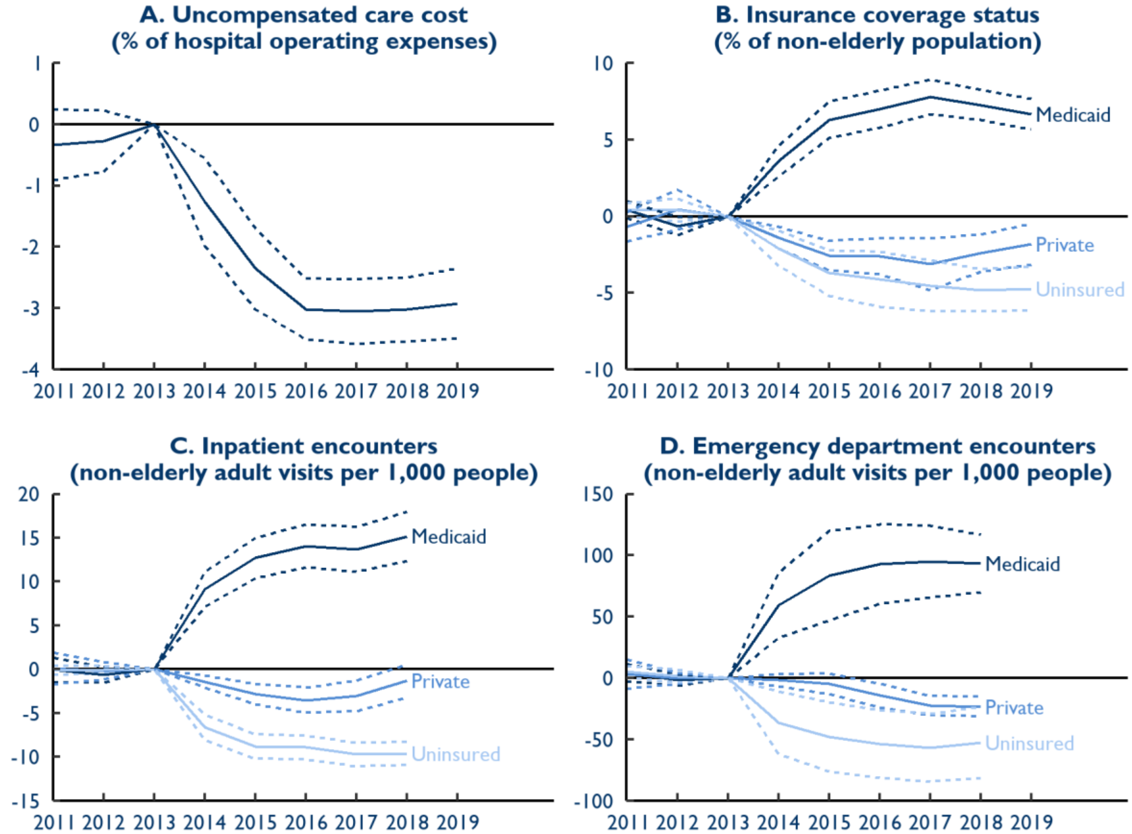
For estimation purposes, I specify the functions α, β, $\{\gamma_r\}$, and $\{\tau_r\}$ as linear functions of the baseline uninsured rates u_s^{2013}. Figure A1 suggests that this functional form is likely to do a reasonable job of capturing the variation in the data, while limiting the number of free parameters. Like equation (1), I estimate equation (2) by ordinary least squares and cluster standard errors by state.

2.2 Results

Table A1 summarizes the estimates obtained by estimating equations (1) and (2).

I begin by examining whether post-2013 trends do in fact differ based on states' baseline uninsured rates. Formally, I test the hypothesis that the functions $\{\gamma_r\}$ and $\{\tau_r\}$ from equation (2) are constant functions.

Figure A2. Predicted Effect of Medicaid Expansion in Coverage Gap States



Note: Plotted amounts are obtained by using the fitted equation (2) to predict the average effect of expansion in the coverage gap states for each year, weighting each state by the denominator of the relevant outcome in that state in 2019. Dashed lines depict 90% confidence intervals.

I run a joint test for the final 3 years with data for the relevant outcome (i.e., 2016-2018 for the HCUP outcomes and 2017-2019 for the other outcomes) since I am ultimately interested in the long-run effect of expansion. The p-values from these tests are reported in the final column of Table A1. For most outcomes, I strongly reject the hypothesis that post-2014 trends are identical in states with different baseline uninsured rates, consistent with the patterns depicted in Figure A1. This provides a strong rationale to prefer the estimates from equation (2) to those from equation (1).

I next examine how the predicted causal effects differ across specifications. For each specification, I obtain a predicted causal effect for each state and year, and I then average these effects across the 11 coverage gap states and over the final 3 years for which data are available for the outcome in question. I focus on the coverage gap states since those are the causal effects of interest here, while averaging over 3 years modestly increases statistical precision. In computing the average for each outcome, I weight each state according its denominator for that outcome (i.e., aggregate hospital operating expenses for the uncompensated care outcomes, non-elderly population for the insurance outcomes, and non-elderly adult population for the utilization outcomes) in the final year of data and weight the 3 years equally.

Comparing the estimates in the second and fourth columns of table A1 indicates that the estimated average causal effects obtained from the conditional event study specification in equation (2) are almost always larger than the estimates obtained from equation (1). For many of the key outcomes, including the changes in uncompensated care, Medicaid enrollment, uninsurance, and Medicaid and uninsured utilization, the estimates from derived from equation (2) are larger by around 50% or more.

To provide some insight into the dynamics of the estimated effects, Figure A2 plots the predicted average causal effects from equation (2) for each year, averaging once again across the coverage gap states and weighting each state by the denominator of the relevant outcome in the final year of the data. In general, the effects of expansion appear immediately after 2014 and roughly stabilize by 2016 or 2017. The "placebo" effects estimated for 2011 and 2012 are uniformly very close to zero, which provides some support for the common trends assumptions required for equation (2) to generate valid causal estimates.

3 Detail on Methods for Estimating the Effect of Filling the Coverage Gap

This section provides additional details on the methods I use to estimate how filling the Medicaid coverage gap would affect hospital finances. I first describe how I estimate spending per coverage gap program enrollee for various prices that might be paid for those enrollees' care; these estimates are used in various places in the subsequent calculations. I then describe how I calculate the effect on hospital finances that would arise through each of the four channels that were discussed in the main text: (1) payment for previously uncompensated care; (2) profits on new volume; (3) lost revenue from shifts out of higher-paying forms of coverage; and (4) automatic reductions in Medicare DSH payments.

3.1 Spending per coverage gap program enrollee at various provider prices

This section describes how I estimate hospital spending per person enrolled in a coverage gap program for various prices that might be paid for those enrollees' care. My starting point is projections of federal benefit spending per Medicaid expansion enrollee from the Congressional Budget Office's most recent baseline projections (CBO 2021) in light of my maintained assumption that utilization patterns under a coverage gap program would resemble those under expansion. Taking a 75/25 weighted average of the estimates for fiscal years 2023 and 2024 and dividing by 0.9 to account for the 10% state share, I estimate that *total* benefit spending per Medicaid expansion enrollee will be $7,694 in calendar year 2023.

Next, I estimate what share of this amount will ultimately be spent on health care, whether by states directly under their fee-for-service programs or indirectly via managed care plans. To do so, I first exclude the portion of payments to Medicaid managed care plans that goes to purposes other than paying claims. Researchers at Milliman estimate that Medicaid managed care plans had an average MLR of 89% in 2019 (Palmer et al. 2021), while MACPAC (2020) estimates that 75% of benefit spending on the expansion population went to managed care plans in fiscal year 2018. Using these estimates, I arrive at a projection of total claims spending per Medicaid expansion enrollee of $7,039 in calendar year 2023.

I then determine what share of that claims spending accrues to hospitals. MACPAC (2020) estimates that 54% of fee-for-service spending on the expansion population went to hospitals in fiscal year 2018, and I assume that managed care plans devote the same share of total claims spending to hospital services.[11] To split these amounts between inpatient and outpatient care, I use the Medicaid financial management reports for fiscal year 2019 to estimate that (across all eligibility categories) 77% of overall fee-for-service spending on hospital services was for inpatient services. Multiplying these percentages by the per enrollee

[11] This assumption could go awry if different types of enrollees are in managed care relative to fee-for-service or if managed care plans change utilization patterns in ways that affect the hospital share. Unfortunately, I am unaware of data on the distribution of spending across service types for expansion enrollees covered in managed care.

Table A2: Spending Per Coverage Gap Program Enrollee, Calendar Year 2023

	Provider Pricing Scenario					
	Medicaid, excluding DSH	Individual market	Federal Medicaid-like Plan	Employer-sponsored insurance	Hospital average cost	Hospital marginal cost
A. Spending ($)						
Inpatient	2,896	4,373	2,970	6,118	3,228	2,750
Outpatient	889	1,407	911	1,968	991	844
All hospital	3,785	5,780	3,881	8,086	4,219	3,594
B. Prices (% of Medicare)						
Inpatient	98	147	100	206	109	93
Outpatient	98	154	100	216	109	93

spending amount derived above yields the spending estimates by service line that are reported in the first column of Table A2, Panel A. I note that all of the data used in the preceding calculations exclude disproportionate share hospital (DSH) payments, so the spending amounts reported in to the first column of Table A2 also exclude those payments, as indicated by the column header.

I now reprice those amounts to reflect spending under the two potential types of coverage gap program, as well as under various other pricing regimes. To that end, I first obtain an estimate Medicaid hospital prices. MACPAC (2017) estimates that Medicaid payments, including all supplemental payments, were 106% of Medicare's, on average, as of 2010. Combining that estimate with an estimate from MACPAC (2021) that DSH payments are currently around 8% of total Medicaid payments to hospitals implies that Medicaid's hospital prices, excluding DSH payments, are 98% of Medicare's prices.

For the other payers, I apply the pricing assumptions described in the main text. Specifically, I obtain estimates of prices in employer-sponsored insurance relative to Medicare from Chernew, Hicks, and Shah (2020), and I estimate prices for individual market plans by scaling those prices down based on the difference in risk-adjusted average claims spending between individual market plans and employer plans reported by Lissenden et al. (2020). I derive my estimates of the prices that would exactly cover hospitals' average cost and marginal cost based on estimates of Medicare margins and marginal profit from MedPAC (2021). The resulting price assumptions are summarized in Table A2, Panel B.

3.2 Payments for Previously Uncompensated Care

As described in the main text, I estimate how much revenue hospitals would gain from payments for previously uncompensated care in two steps. First, I estimate how much reported uncompensated care cost would decline under a coverage gap program. Second, I scale this estimate based on how the prices paid by a coverage gap program compare to hospitals' average cost of delivering care, as reflected in Table A2, Panel A. I describe each step, as well as the conceptual rationale for the second step, below.

For the first step, I use the fitted version of equation (2) to produce state-specific estimates of the change in reported uncompensated care cost as a share of hospital operating expenses for each coverage gap state. I note that, per the adjustments I make to the data in section 1.2, these estimates reflect the reporting definitions and practices in effect prior to the various changes made by CMS in 2016 and 2017.

To convert these changes into dollar amounts, I multiply each state-specific estimate by a projection of hospital operating expenses in that state for 2023. I derive that projection by starting with the corresponding amounts calculated from cost reports for 2019 and then trending them forward according to projected growth in aggregate hospital spending from the 2019 through 2023 in the March 2020 National Health Expenditure projections. Summing across states yields an estimated reduction in reported uncompensated care costs of $9.4 billion if a coverage gap program were fully in effect in 2023.

The question is then how to translate that estimate into an improvement in hospitals' finances. This is more complicated than it might first appear because uncompensated care cost, as it appears on hospital cost reports, is not simply the cost a hospital incurs to deliver uncompensated care. Rather, under the Worksheet S-10 definitions in effect prior to the definitional changes that were described in section 1.2, reported uncompensated care cost can be understood to be defined as follows:[12]

$$U \equiv \underbrace{\theta G_C - P_{C,U}}_{\text{charity care cost, uninsured} \equiv U_{C,U}} + \underbrace{\theta D_C - P_{C,I}}_{\text{charity care cost, insured} \equiv U_{C,I},} + \underbrace{\theta(G_B - P_{B,U} + D_B - P_{B,I})}_{\text{bad debt cost} \equiv U_B}, \qquad (3)$$

where the equation reflects the following definitions: G_C and G_B are, respectively, the hospital's charges for care delivered to uninsured patients that is ultimately categorized as "charity care" or "bad debt"; D_C and D_B are the amounts of cost-sharing associated with services delivered to insured patients that become classified as charity care or bad debt; the amounts $P_{j,k}$ reflect what the hospital is able to collect with respect to care in charity-care/bad-debt category $j \in \{C, B\}$ and insurance status $k \in \{U, I\}$; and θ is the hospital's cost-to-charge ratio. I note that the fact that the cost-to charge ratio is applied to amounts other than the charge amounts G_C and G_B is hard to rationalize, but equation (3) is an accurate reflection of the calculations that were supposed to occur on Worksheet S-10 under this set of definitions.[13]

For convenience, I now define $C_j \equiv \theta G_j$, which is a measure of the cost of delivering care with charges G_j. Additionally, I use the Δ operator to denote the change in a quantity due to implementation of a coverage gap program and let p represent the coverage gap program's prices as a share of hospitals' average cost (i.e. costs inclusive of both fixed and variable costs) of delivering care.

Under these definitions, the improvement in hospital margins from changes in uncompensated care caused by implementation of a coverage gap program, which I denote ΔM, is given by:

$$\Delta M = -p(\Delta C_C + \Delta C_B) - (\Delta D_C + \Delta D_B) + \sum_{\substack{j \in \{C,B\} \\ k \in \{U,I\}}} \Delta P_{j.,k}. \qquad (4)$$

The first term in equation (4) is the revenue hospitals would receive under the coverage gap program for care that was previously categorized as charity care or bad debt. The second term is the reduction in the amount of cost-sharing associated with services that become categorized as charity care or bad debt. The third term nets out the partial payments the hospital was able to collect under the status quo.

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[12] Equation (3) makes the simplification of excluding care delivered to *insured* patients that is not covered by the patient's insurance, which can sometimes be counted as charity care or bad debt. The amount of such care appears to be relatively small. Additionally, the reporting instructions are vague about exactly how bad debt should be calculated, so the equation reflects an assumption about how hospitals interpret those instructions.

[13] The revised definitions eliminate the erroneous application of the cost-to-charge ratio to cost-sharing written off as charity care, but newly create this problem with respect patient collections associated with uninsured charity care services.

If all of the constituent components of equation (3) were reported separately on Worksheet S-10, then each of the causal effects that appear in equation (4) could be estimated directly. Unfortunately, some of these amounts are not reported separately (either ever, or in some years). In light of this issue, I instead estimate the effect on hospital finances as $-p\Delta U$, as described in the main text.

To assess the reliability of this approach, I note that equations (3) and (4) can be used to obtain:

$$-p\Delta U = \Delta M + (p-1)\Delta P_{C,U} - (p\theta - 1)\Delta D_C + (p-1)\Delta P_{C,I} + (p\theta - 1)\left[\Delta P_{B,U} - \Delta D_B + \Delta P_{B,I}\right]. \quad (5)$$

That is, $-p\Delta U$ is equal to the desired effect on hospital margins, plus several bias terms. The bias terms arise for two reasons. First, multiplying ΔU by the price p inappropriately scales the entire amount by the price p, rather than just the ΔC_j terms. Second, as discussed in the paragraph following equation (3), the uncompensated care calculation anomalously discounts several amounts by the cost-to-charge ratio θ, which then causes those amounts to be inappropriately discounted in the change ΔU.

To gauge the size of these bias terms, I require estimates of cost-sharing changes ΔD_j and the patient collection changes $\Delta P_{j,k}$. To estimate $\Delta P_{C,U}$, I assume that it moves in proportion with the overall change in charity care cost for uninsured people; formally, I assume that $\Delta P_{C,U} = \left[P^0_{C,U}/U^0_{C,U}\right]\Delta U_{C,U}$, where the zero superscripts indicate baseline amounts prior to implementation of a coverage gap program. Similarly, I estimate ΔD_C and $\Delta P_{C,I}$ by assuming that these changes are in proportion to the overall change in charity care cost for insured people; that is, I assume $\Delta D_C = \left[D^0_C/U^0_{C,I}\right]\Delta U_{C,I}$ and $\Delta P_{C,I} = [P^0_{C,I}/U^0_{C,I}]\Delta U_{C,I}$. Estimates of the causal effects required to implement these formulas appear in Table A1. Each relevant scaling factor can be estimated using data reported on Worksheet S-10 (lines 20 and 22) for years in which the old definitions were in effect; I use calendar year 2012 for these calculations.

A similar approach is not possible for the corresponding bad-debt-related changes $\Delta P_{B,U}$, ΔD_B, and $\Delta P_{C,U}$ because Worksheet S-10 has never disaggregated the components of bad debt. Instead, I simply scale the estimated causal effects for the charity care category according to a measure of the relative size of the charity care and bad debt categories at baseline. Formally, for a change ΔX_B that pertains to the bad debt category and the corresponding change ΔX_C that pertains to the charity care category, I assume $\Delta X_B = \Delta X_C\left[G^0_B - P^0_{B,U} + D^0_B - P^0_{B,I}\right] / [G^0_C - P^0_{C,U} + D^0_C - P^0_{C,I}]$. Data from Worksheet S-10 (lines 20-23 and line 28) can be used to estimate both the numerator and the denominator of the scaling factor in this equation in years where the old definitions are in effect; to do so, I again use data for calendar year 2012.

The only parameter that remains to be specified is the cost-to-charge ratio θ. Inspection of hospital-level data suggests that hospitals were inconsistent in applying the cost-to-charge ratio to D_C, so the effective cost-to-charge ratio applied to these amounts was smaller than the ratio applied to the various bad debt terms. Thus, I use different estimates of θ in these two cases. Again, I rely on Worksheet S-10 data for calendar year 2012. For calculations involving ΔD_C, I obtain an estimate $\theta = 0.564$ from lines 20 and 21, column 1. For the bad debt terms, I obtain an estimate $\theta = 0.265$ from lines 28 and 29.

Table A3 reports the resulting bias estimates. The first column reports the estimate of each causal effect that appears in equation (4). The next two columns report coefficients that apply to each term (e.g., $p - 1$ or $p\theta - 1$) when p has the value it would have under the relevant program type (per the estimates from Table A2). The final two columns report the resulting estimates of each bias term in equation (4).

Table A3: Estimates of Bias Terms in Equation (5)

Term	Estimated Effect (% of operating expenses)	Coefficient		Bias Amount (% of operating expenses)	
		Federal Medicaid plan	Marketplace-based program	Federal Medicaid plan	Marketplace-based program
$\Delta P_{C,U}$	-0.09	-0.08	0.37	0.01	-0.03
ΔD_C	-0.35	0.48	0.23	-0.17	-0.08
$\Delta P_{C,I}$	-0.02	-0.08	0.37	0.00	-0.01
$\Delta P_{B,U}$	-0.07	-0.76	-0.64	0.05	0.04
ΔD_B	-0.26	0.76	0.64	-0.19	-0.16
$\Delta P_{B,I}$	-0.02	-0.76	-0.64	0.01	0.01
Net	N/A	N/A	N/A	-0.29	-0.23

The estimates suggest that the net bias from using $-p\Delta U$ as my estimate of the effect a coverage gap program would have on hospital finances through the uncompensated care channel is relatively small. For either type of coverage gap program, the estimated bias falls in a range between 0.2% and 0.3% of hospital operating expenses or $0.7-0.9 billion. While there is uncertainty around this estimate, as the method I use to estimate the bias is imperfect, these estimates do suggest that any bias is likely relatively modest.

Finally, there is one other potential source of bias that is not captured in the calculations above. As discussed in section 1.2, I adjust the raw uncompensated care cost data to reflect the reporting definitions and practices in effect before the series of changes CMS made in 2016 and 2017. However, one of the definitional changes CMS made during that period was to clarify that instances where a hospital provided a discount to an uninsured patient (but did not waive payment entirely) could be included when tallying charity care. The net bias from this exclusion (to the extent hospitals were in fact excluding these amounts when reporting charity care prior to the CMS changes) is unclear, however. While including the cost of this care in uncompensated care cost could lead me to estimate larger declines in charity care due to Medicaid expansion, excluding the *payments* associated with that care (which are, by definition, likely more substantial in cases where hospitals grant partial discounts) could work in the opposite direction.

3.3 Profits on New Volume

As described in the main text, I estimate hospital profits from the increase in utilization spurred by the introduction of a coverage gap program in two steps. In the first step, I use the difference-in-differences estimates derived above to estimate what share of the hospital services paid for by a coverage gap program would represent new utilization. In the second step, I multiply those shares by an estimate of the difference between: (1) the aggregate amount the coverage gap program would spend on hospital care; and (2) the aggregate (marginal) costs hospitals would incur to deliver that care.

Starting with the first step, I use the fitted version of equation (2) to produce state-specific estimates of the change in Medicaid, uninsured, and private payer inpatient and emergency department utilization that would occur in each coverage gap state under Medicaid expansion. I then compute mean effects across the coverage gap states as a whole, weighting each state by its age 19-64 population as of 2019 (as calculated from the American Community Survey), and I calculate the share of utilization that would represent new utilization by dividing the sum of the estimated effects for Medicaid, uninsured, and private insurance by the estimated effect for Medicaid. Applying my maintained assumption regarding the the equivalence between a coverage gap program and Medicaid expansion, I assume that the same share would apply to a coverage gap program. The resulting shares are reported in Panel A of Table 2.

Turning to the second step, the calculations are largely described in the main text, except for how I estimate total enrollment in a coverage gap program. To derive that estimate, I use equation (2) to produce state-specific estimates of the change in the share of the non-elderly population that would be enrolled in Medicaid under Medicaid expansion. To convert those shares to estimated numbers of enrollees, I multiply by the state's non-elderly population as of 2019, trended forward to 2023 based on the (national) population projections in the March 2020 National Health Expenditure projections. Summing the resulting estimates across states yields an increase in Medicaid enrollment of 5.8 million. Applying my maintained assumption regarding the equivalence between a coverage gap program and Medicaid expansion, I adopt this as my estimate of total enrollment in a coverage gap program.

3.4 Transitions from Other Forms of Coverage to the Coverage Gap Program

Mirroring my approach for estimating the effects of increases in utilization, I estimate the effect of shifts across coverage types in two steps. In the first step, I use the difference-in-differences estimates derived above to estimate the fraction of utilization under the coverage gap program that represents a shift out of other forms of coverage. In the second step, I multiply those shares by an estimate of the difference between: (1) the aggregate amount the coverage gap program would spend on hospital care; and (2) the aggregate amount that would be spent on that care if the care was paid for at alternative prices (either individual-market or employer-market prices, depending on the type of shifting in question).

Starting with the first step, I use the fitted version of equation (2) to produce state-specific estimates of the change in inpatient and emergency department utilization by payer that would occur in each coverage gap state under Medicaid expansion. The underlying HCUP data aggregate all private payer utilization, so it is necessary to split apart the change in individual market and employer plan utilization.

To that end, I turn to my estimates of how expansion affects *insurance coverage*, which do disaggregate individual market and employer plan enrollment. Specifically, I use the fitted version of equation (2) to produce state-specific estimates of the effect of expansion on the share of the population with individual market and employer-sponsored insurance. I then allocate the estimated change in private payer *utilization* for each state across these two types of private coverage in proportion to these estimated changes in *coverage*. Paralleling my approach above, I then compute mean utilization effects across the full set of coverage gap states, weighting each state by its age 19-64 population as of 2019.

With these mean effects in hand, I calculate the ratio of the estimated change in utilization in employer-sponsored plans to the estimated change in Medicaid utilization; I adopt this as my estimate of the shift of utilization out of employer-sponsored coverage under either type of coverage gap program. For the scenario with a federal Medicaid plan, I estimate shifting out of the individual market share by taking the ratio of the estimated change in utilization in individual market plans to the estimated change in Medicaid utilization. The remainder of the calculations are as I describe in the main text.

3.5 Automatic Changes in Medicare DSH Payments

The methodology I use to estimate changes in Medicare DSH payments is largely described in the main text. Here, I provide additional detail on how I estimate two things: (1) the proportional reduction in the national uninsured rate due to a coverage gap program; and (2) the distribution of uncompensated care across states under current law and with a coverage gap program. I discuss each in turn.

To estimate the proportional reduction in the national uninsured rate, I consider the under-65 and 65-plus populations separately. For the under-65 population, I derive estimates of the number of uninsured under current law and with a coverage gap program. For the current law scenario, I assume that a state's uninsured rate in 2023 will match its uninsured rate in 2019 (as estimated in the ACS) unless the state has expanded Medicaid in the interim; I make a downward adjustment for expanding states using the predicted effect of expansion from equation (2). For the scenario with a coverage gap program, I start with the current law estimates and then reduce the uninsured rate in the coverage gap states using the predicted effect of expansion from equation (2), consistent with my assumption that a coverage gap program would be equivalent to expansion. I then multiply these estimated uninsured rates by a projection of each state's under-65 population in 2023, which I derive by starting with the state's under-65 population as of 2019 (as reported in the ACS) and trending it forward according to projected national growth in the under-65 population CMS' March 2020 National Health Expenditure projections.

For the 65-plus population, I assume that the uninsured rate is unaffected by the coverage gap program. Additionally, I assume that each state's uninsured rate in this age group will match its uninsured rate as of 2019 (as estimated in the ACS). I then multiply these estimated uninsured rates by a projection of each state's 65-plus population, derived in the same way as the under-65 population estimates above.

I use essentially the same approach to estimate the distribution of uncompensated care under current law and with a coverage gap program. For the current law scenario, I assume that uncompensated care cost will constitute the same share of hospital operating expenses in 2023 as it did in 2019 unless the state has expanded Medicaid in the interim; for expanding states, I make a downward adjustment using the predicted effect of expansion from equation (2). For the scenario with a coverage gap program, I start with the current law estimates and reduce the amount of uncompensated care in the coverage gap states using the predicted effect of Medicaid expansion from equation (2). To convert these estimates to dollar amounts, I multiply the estimated shares by a projection of hospital operating expenses in each state as of 2023, which I derive by starting with the corresponding amounts calculated from cost reports for 2019 and then trending them forward according to projected growth in aggregate hospital spending from the 2019 through 2023 in the March 2020 National Health Expenditure projections

I note that the resulting estimates reflect uncompensated care cost under the reporting definitions and practices in effect prior to the various change made by CMS in 2016 and 2017 described in section 1.2. However, for these purposes, I am interested in uncompensated care cost under current definitions and reporting practices. Thus, in a final step, I reverse the adjustment described in section 1.2.

4 References

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