

March 2021

# Enrollment in Nongroup Health Insurance by Income Group

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USC-Brookings Schaeffer Initiative for Health Policy

This report is available online at: <https://www.brookings.edu/research/enrollment-in-nongroup-health-insurance-by-income-group/>

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## **EDITOR'S NOTE**

This white paper is part of the USC-Brookings Schaeffer Initiative for Health Policy, which is a partnership between the Economic Studies Program at Brookings and the USC Schaeffer Center for Health Policy & Economics. The Initiative aims to inform the national health care debate with rigorous, evidence-based analysis leading to practical recommendations using the collaborative strengths of USC and Brookings. The Robert Wood Johnson Foundation provided a grant to the Brookings Institution that supported the writing of this paper.

## **ACKNOWLEDGEMENTS**

I thank Loren Adler, Paul Ginsburg, and Jason Levitis for helpful comments and conversations. I thank Conrad Milhaupt for excellent research assistance, and Brianna Nicker for excellent editorial assistance. All errors are my own.

## Executive Summary

In the United States, most people obtain health insurance either through an employer or a public program like Medicare or Medicaid. But some people—about 11% of the non-elderly population in 2019 according to this paper’s estimates—lack access to public or employer coverage. These people must instead seek nongroup coverage, either through the Marketplaces established by the Affordable Care Act (ACA) or outside the Marketplaces. For many purposes, including assessing proposals to increase nongroup enrollment such as the recently enacted increases in the ACA’s subsidies for Marketplace plans, it is useful to know how many people have nongroup coverage, what forms of nongroup coverage they have, how many potential enrollees remain unenrolled, and how enrollment rates vary by income.

To that end, this paper estimates how many non-elderly people in different income groups held Marketplace coverage, off-Marketplace coverage that qualifies as minimum essential coverage (MEC) under the ACA, or non-MEC nongroup policies (e.g., short-term limited duration plans), as well as how many lacked any coverage, in 2019. I focus on enrollment among people who: (1) are ineligible for public or employer coverage; (2) are legally present in the United States; and (3) do not fall in the Medicaid “coverage gap.” This group, which I refer to as the population of “potential subsidy recipients,” is the group most likely to be targeted by efforts to increase nongroup enrollment such as the recently enacted subsidy expansions. Thus, estimates for this population are particularly policy relevant.

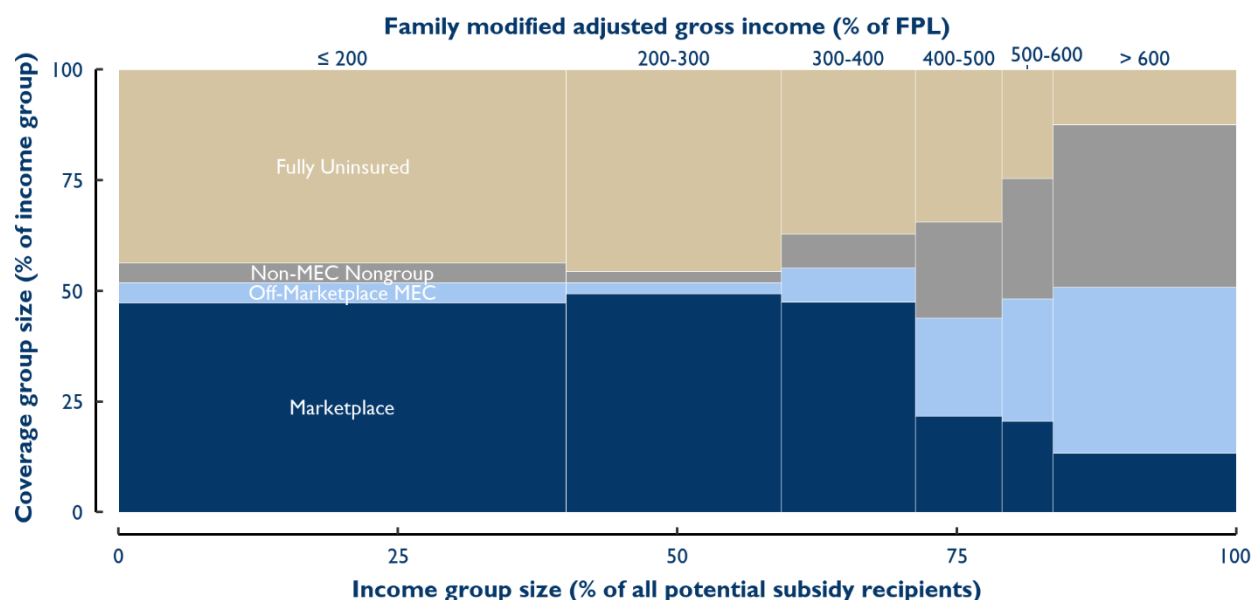
Estimating patterns of nongroup enrollment is challenging. Household surveys do a poor job of measuring nongroup coverage since respondents often report other forms of coverage as nongroup coverage and vice versa (Pascale, Fertig, and Call 2019), while administrative data lack needed detail. Thus, this paper produces estimates by blending administrative and survey data. In brief, I start with tabulations of insurance coverage by income in 2016 produced using tax data by Lurie and Pearce (2019; forthcoming), which likely offer the best available snapshot of nongroup enrollment by income. I then use a combination of survey, administrative, and other data to construct crude estimates of enrollment in non-MEC nongroup policies (which are not captured in the tax data), trend the various estimates forward to 2019, and make other needed adjustments. In doing so, I pay careful attention to the limitations of the survey data sources and mitigate those limitations to the greatest extent possible.

### Findings Regarding Patterns of Nongroup Enrollment in 2019

Figure ES.1 summarizes this paper’s estimates of nongroup enrollment and uninsurance among non-elderly potential subsidy recipients in 2019. The paper highlights four principal findings, each of which has important implications for policymakers, researchers, or both.

**Finding #1: Only half of potential subsidy recipients were enrolled in nongroup MEC, and take-up rates varied only modestly with income.** Slightly more than half (52%) of those with incomes below 400% of the federal poverty level (FPL), the income limit for the ACA’s Marketplace subsidies as of 2019, were enrolled in nongroup policies that constitute MEC. This fraction was slightly lower (49%) among people with incomes above 400% of the FPL. These estimates indicate that there was considerable scope to increase enrollment in nongroup MEC at all income levels, including at income levels where Marketplace subsidies were already available as of 2019. This implies that the recently enacted American Rescue Plan Act, which both made subsidies more generous for people already eligible and extended them above 400% of the FPL, has the potential to increase MEC enrollment across the income distribution.

**Figure ES.1: Potential Subsidy Recipients by Income and Coverage, 2019**



Note: Potential subsidy recipients are people who are legally present, ineligible for public and employer coverage, and not in the Medicaid "coverage gap." Estimates include people under age 65 only. Enrollment measured in life-years. MEC = minimum essential coverage.

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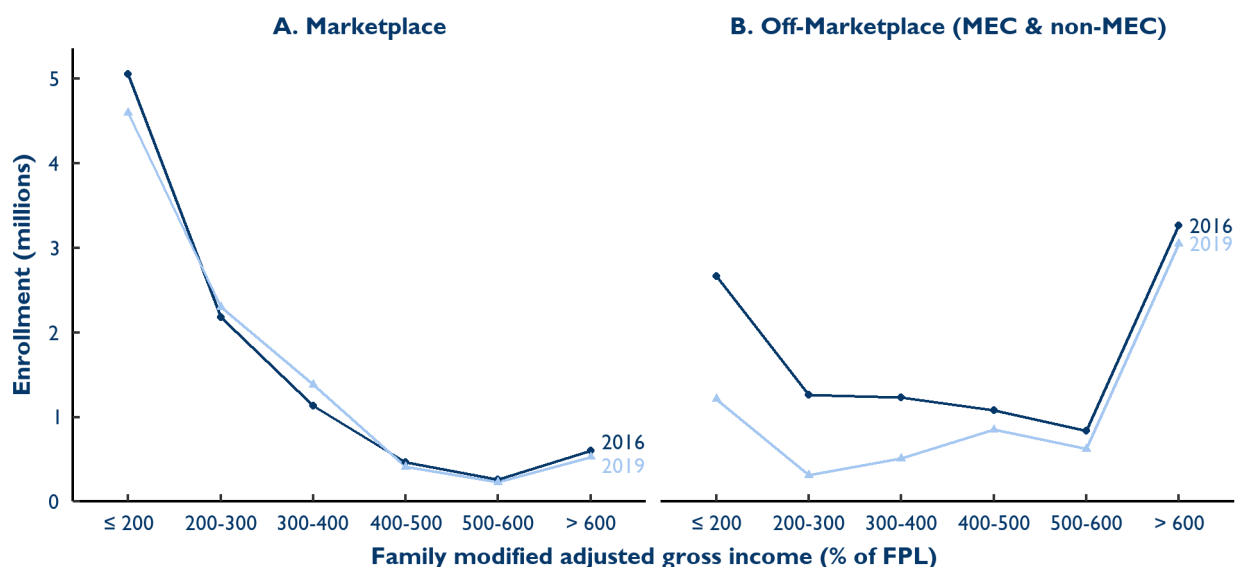
**Finding #2: The large majority of potential subsidy recipients who lacked MEC had incomes below 400% of the FPL.** Among potential subsidy recipients who lacked nongroup MEC, 70% had incomes below 400% of the FPL. Thus, not only is there scope to increase enrollment in nongroup MEC among people who were already eligible for subsidies, most of the opportunity to increase overall take-up is in this group.

**Finding #3: At lower income levels, potential subsidy recipients who lacked MEC were typically uninsured, while at higher incomes many held non-MEC nongroup policies.** Among potential subsidy recipients without nongroup MEC who had incomes below 400% of FPL, 91% were fully uninsured, while the remaining 9% held non-MEC nongroup policies. By contrast, at higher income levels, 61% held non-MEC nongroup policies and only 39% were fully uninsured. This suggests that efforts to increase MEC enrollment may do less to improve financial protection at higher income levels, although those improvements may still be substantial since non-MEC policies often offer much less robust coverage (e.g., Pollitz et al. 2018; Palanker, Curran, and Salyards 2020). It also suggests that efforts to restrict the availability of non-MEC policies would have their largest effects at higher income levels.

**Finding #4: Lower-income potential subsidy recipients with MEC overwhelmingly held Marketplace coverage, while higher-income people generally held off-Marketplace MEC.** Among potential subsidy recipients with nongroup MEC, 91% of those with incomes below 400% of the FPL held Marketplace coverage, whereas just 35% did above 400% of the FPL. One implication is that research that focuses only on Marketplace enrollees can paint a very misleading picture of nongroup enrollment as a whole.

Since subsidies were available below 400% of the FPL, it is notable that anyone at these income levels opted for off-Marketplace plans. This could indicate that awareness of subsidized coverage is incomplete, as some surveys suggest (Gupta and Collins 2019; Pollitz et al. 2020). Additionally, underwritten off-Marketplace policies (grandfathered and transitional MEC policies and non-MEC policies) may sometimes be less expensive than subsidized Marketplace coverage for healthier moderate-income enrollees.

**Figure ES.2: Estimated Nongroup Enrollment, 2016 and 2019**



Note: Estimates include people under age 65 only. Enrollment measured in life-years. MEC = minimum essential coverage.

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### Findings Regarding Trends in Nongroup Enrollment from 2016 to 2019

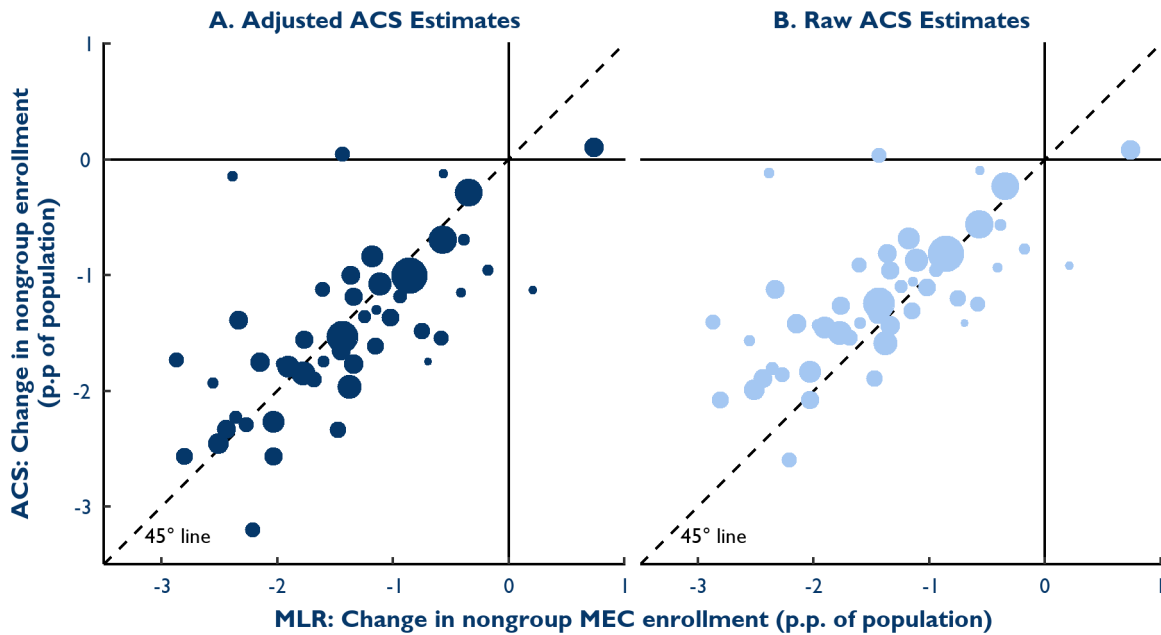
The method this paper uses to estimate enrollment in 2019 also generates estimates of how different categories of enrollment changed from 2016 to 2019. The paper highlights two main findings.

**Finding #5: Off-Marketplace enrollment fell sharply over this period, particularly at lower income levels, while Marketplace enrollment was steady.** Figure ES.2 depicts these trends graphically. The decline in off-Marketplace enrollment is unsurprising since premiums of ACA-compliant policies rose sharply over this period (e.g., CMS 2019) and since some healthier people with underwritten “grandfathered” or “transitional” policies churned out of the market over time and likely were not replaced. However, to the extent this decline was driven by rising premiums, it is somewhat surprising that there was no offsetting increase in Marketplace enrollment at incomes below 400% of the FPL, where subsidies were available. This could indicate that people exiting off-Marketplace plans were unaware that subsidized Marketplace coverage existed. Alternatively, it could indicate that Marketplace enrollment was stable in the aggregate because an influx of former off-Marketplace enrollees was offset by other factors that reduced Marketplace enrollment, such as the elimination of the individual mandate penalty.

**Finding #6: Enrollment in non-MEC nongroup policies may have been steady from 2016 to 2019.** Existing data sources do not directly measure trends in non-MEC nongroup enrollment. To fill this gap, this paper constructs an indirect measure of those trends by combining data from insurer Medical Loss Ratio (MLR) filings with data from the American Community Survey (ACS). Because the MLR data only capture enrollment in nongroup MEC, while the ACS plausibly captures all nongroup enrollment, any difference in the enrollment trends shown in the MLR and ACS data may reflect changes in non-MEC enrollment.

Figure ES.3 shows that estimates of the change in nongroup enrollment from 2016 to 2019 derived from the MLR and ACS data are very highly correlated at the state level. Furthermore, after using the results of Pascale, Fertig, and Call (2019) to adjust for known patterns of coverage misreporting in the ACS, the ACS

**Figure ES.3: ACS vs. MLR Nongroup Enrollment Trends by State, 2016-2019**



Note: Marker size reflects state population in 2016. Plot excludes New Hampshire, which experienced an idiosyncratic large decline in MLR-measured enrollment because it stopped covering Medicaid expansion enrollees through nongroup plans during this period. ACS = American Community Survey; MLR = medical loss ratio; MEC = minimum essential coverage.

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and MLR data show very similar enrollment trends, on average across states. This finding suggests that enrollment in non-MEC nongroup policies may have changed little over this period.

Stable enrollment in non-MEC policies may be the net effect of offsetting factors. On the one hand, premium increases for ACA-compliant plans (e.g., CMS 2019), elimination of the individual mandate policy, and federal regulatory changes favoring short-term limited duration plans may have increased non-MEC enrollment. However, many states implemented new restrictions on short-term plans during this period (Palanker, Kona, and Curran 2019), which may have worked in the opposite direction.

### Opportunities to Improve Nongroup Enrollment Data

The paper closes by making two recommendations for how data on enrollment in nongroup coverage could be improved in the future. First, the Treasury Department should routinely publish tabulations of insurance coverage by income group based on tax data like those produced by Lurie and Pearce (2019; forthcoming) and consider publishing estimates that are further disaggregated by state of residence.

Second, policymakers should collect better data on enrollment in non-MEC nongroup policies. The best approach would be to extend the tax reporting regime that applies to MEC policies to non-MEC policies. Encompassing all types of non-MEC nongroup policies would likely require legislation, but it might be possible to do so for short-term limited duration policies via administrative action. If extending the tax reporting regime is not feasible, it would be valuable to at least collect aggregate information on non-MEC nongroup enrollment. The National Association of Insurance Commissioners has collected some data on enrollment in short-term limited duration policies and may collect more, but these efforts should be extended to other types of non-MEC nongroup policies. Additionally, these data should be made broadly available to researchers and policymakers, which is not currently planned (Keith 2020).

## Introduction

In the United States, most people obtain health insurance either through an employer or a public program like Medicare or Medicaid. But some people—about 11% of the non-elderly population in 2019 according to this paper’s estimates—lack access to public or employer coverage. These people must instead seek nongroup coverage, either through the Marketplaces established by the Affordable Care Act (ACA) or outside the Marketplaces. For many purposes, including assessing proposals to increase nongroup enrollment such as the recently enacted increases in the ACA’s subsidies for Marketplace plans, it is useful to know how many people have nongroup coverage, what forms of nongroup coverage they have, how many potential enrollees remain unenrolled, and how enrollment rates vary by income.

To that end, this paper estimates how many non-elderly people in different income groups held Marketplace coverage, off-Marketplace coverage that qualifies as minimum essential coverage (MEC) under the ACA, or non-MEC nongroup policies (e.g., short-term limited duration plans), as well as how many lacked any coverage, in 2019. I focus on enrollment among people who: (1) are ineligible for public or employer coverage; (2) are legally present in the United States; and (3) do not fall in the Medicaid “coverage gap.”<sup>1</sup> This group, which I refer to as the population of “potential subsidy recipients,” is the group most likely to be targeted by efforts to increase nongroup enrollment such as the recently enacted subsidy expansions.<sup>2</sup> Thus, estimates for this population are particularly policy relevant.

Estimating patterns of nongroup enrollment is challenging. Household surveys do a poor job of measuring nongroup coverage since respondents often report other forms of coverage as nongroup coverage and vice versa (Pascale, Fertig, and Call 2019), while administrative data lack needed detail. Thus, this paper produces estimates by blending administrative and survey data. In brief, I start with tabulations of insurance coverage by income in 2016 produced using tax data by Lurie and Pearce (2019; forthcoming), which likely offer the best available snapshot of nongroup enrollment by income. I then use a combination of survey, administrative, and other data to construct crude estimates of enrollment in non-MEC nongroup policies (which are not captured in the tax data), trend the various estimates forward to 2019, and make other needed adjustments. In doing so, I pay careful attention to the limitations of the survey data sources and mitigate those limitations to the greatest extent possible.

This paper is related to earlier work that has disaggregated the uninsured population according to their eligibility for various forms of subsidized coverage or estimated take-up of subsidized Marketplace coverage (Blumberg et al. 2018; CBO 2020a; KFF 2021; KFF 2020a) and builds on that work in various ways. This paper makes two main contributions relative to this prior research. First, my estimates offer a

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<sup>1</sup> A person falls in the Medicaid “coverage gap” if the person is ineligible for subsidized Marketplace coverage by virtue of having income below 100% of the FPL but does not qualify for Medicaid. A coverage gap only exists in states that have declined the ACA’s Medicaid expansion since Medicaid eligibility extends to (at least) 138% of the FPL in Medicaid expansion states.

<sup>2</sup> A small number of people eligible for Marketplace subsidies fall outside my definition of “potential subsidy recipient”: people who are offered employer coverage that is not “affordable” (which, in 2019, meant that the family’s required contribution for self-only coverage exceeded 9.86% of income) or that does not provide “minimum value” (meaning that the coverage fails to cover at least 60% of expected medical costs). The limitations of the data sources used in this paper make it challenging to reliably identify people in this category.

comprehensive breakdown of how many people hold each form of nongroup coverage at each income level, as well as how many are uninsured, which prior research does not.<sup>3</sup> Second, my estimates rely more heavily on administrative data sources, particularly tax data, which may make them more accurate.

The remainder of the paper proceeds as follows. I begin by providing some background on the different types of nongroup coverage and their prevalence in 2019. I then describe the Lurie and Pearce estimates, as well as the various other survey and administrative data I use. Next, I describe my methodology and present my results. I conclude with some recommendations on how policymakers could facilitate collection of better data on nongroup enrollment. Appendices provide additional methodological detail.

## Background on Types of Nongroup Coverage

For the purposes of this paper, I place nongroup policies into three different categories: (1) Marketplace coverage; (2) off-Marketplace individual market minimum essential coverage (MEC); and (3) non-MEC nongroup policies. I describe each of these categories in turn below.

### Marketplace coverage

This category consists of individual market policies sold through the ACA's Marketplace. CMS' Marketplace effectuated enrollment reports (which are described further below) indicate that there were 9.8 million life-years of such coverage in 2019, as illustrated in Figure 1.

### Off-Marketplace individual market MEC

This category consists of individual market policies that are sold outside the Marketplaces but nevertheless constitute MEC under the ACA.<sup>4</sup> This includes both ACA-compliant individual market policies (that is, policies that comply with the ACA's requirements for individual market policies, including community rating, guaranteed issue, and essential health benefit requirements) that are sold outside the Marketplaces and grandfathered and transitional individual market plans (which are exempt from many ACA requirements). CMS administrative data indicate that there were 3.6 million life-years of such coverage in 2019, of which 63% was in ACA-compliant policies.<sup>5</sup> Where there is no risk of confusion, I generally refer to these policies as "off-Marketplace MEC."

### Non-MEC nongroup policies

This category consists of people who hold other types of policies that are sold to individuals but that do not constitute MEC. This category includes short-term limited duration policies, various excepted benefits policies (e.g., fixed indemnity coverage), and certain insurance-like products that are not formally

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<sup>3</sup> The Congressional Budget Office's estimates come closest to mine in the amount of detail they provide, but they are not limited to potential subsidy recipients and do not disaggregate different types of nongroup coverage.

<sup>4</sup> The definition of MEC serves multiple functions in federal law. Most importantly, however, it delineated what forms of coverage satisfied the ACA's now-toothless individual mandate.

<sup>5</sup> I estimate enrollment in off-Marketplace MEC by comparing the enrollment reported in CMS' Medical Loss Ratio data to enrollment reported in CMS' Marketplace effectuated enrollment reports; both data sources are described further below. The share of this coverage that is in ACA-compliant policies was estimated using CMS' risk adjustment annual reports, which provide aggregate ACA-compliant enrollment by state. These reports do not include useable data for Massachusetts and Vermont, so I assume that all individual market coverage in these states was ACA-compliant, which prior work (e.g., Fiedler 2017) suggests is a very good approximation.



**Figure 1: Nongroup Enrollment by Policy Type, 2019**



Note: Enrollment measured in life-years. MEC = minimum essential coverage.

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regulated as insurance (e.g., health care sharing ministries).<sup>6</sup> For the purposes of this paper, I am interested only in policies that enrollees may view as substitutes for traditional health insurance, not policies that supplement some other form of coverage (e.g., Medigap policies) or policies that cover a narrow set of services (e.g., dental and vision plans).

These policies are exempt from the requirements that apply to traditional insurance policies and often differ in important ways. Notably, these policies can exclude major categories of services, exclude services related to pre-existing medical conditions, or set dollar limits on how much care they will cover. They can also deny coverage or vary premiums based on health status. While there are no comprehensive data on the characteristics of these policies, it is clear that many offer much more limited financial protection and access to care than other forms of health insurance (e.g., Pollitz et al. 2018; Palanker, Curran, and Salyards 2020). For this reason, the Congressional Budget Office (CBO) treats some people that hold these types of policies as being uninsured in its reports on insurance coverage (CBO 2020b).

There are no systematic data on how many people are enrolled in non-MEC nongroup policies. The lack of data in this area is a major challenge for my analysis, and I discuss how I handle it below.

## Data

This section describes the administrative data and household survey data I use to produce this paper's estimates. I begin by describing the estimates of coverage by income group in 2016 produced using tax records by Lurie and Pearce (2019; forthcoming), which form the core of my analysis. I then describe various other survey and administrative data sources that I use to trend those estimates forward to 2019 and make various other needed adjustments. At the end of the section, I also touch briefly on the limitations of household survey data that make it inadvisable to rely solely on survey data for this analysis.

### Overview of the Lurie and Pearce Estimates

Under the ACA, all providers of MEC must report annually to the Internal Revenue Service (IRS) on who they covered in each month of the year.<sup>7</sup> The definition of MEC encompasses almost all types of insurance

<sup>6</sup> Linke Young (2020) provides a comprehensive introduction to the various forms of insurance or insurance-like products that operate largely or entirely outside of the federal regulatory framework.

<sup>7</sup> See 26 U.S.C. § 6055. The ACA's implementing regulations provide that the Marketplace, rather than the coverage provider is responsible for reporting Marketplace coverage. See 26 C.F.R. § 1.6055-1(d)(1).

coverage, including employer-sponsored coverage, most nongroup coverage, and all major forms of public coverage (including Medicare, Medicaid, and veterans coverage).<sup>8</sup> Coverage providers are also required to report what type of MEC each person holds. Thus, in principle, these data offer a nearly comprehensive and quite detailed picture of coverage patterns in the United States.

Lurie and Pearce (2019; forthcoming) use the 2015 and 2016 vintages of these data to produce tabulations of insurance coverage by family (tax unit) modified adjusted gross income as a percentage of the federal poverty level (FPL) for the relevant calendar year.<sup>9</sup> They obtain data on calendar year income from tax returns or, for people who do not file tax returns, from various information returns received by the IRS (e.g., W-2s).<sup>10</sup> Modified adjusted gross income is the income concept used to determine eligibility for Marketplace subsidies, Medicaid, the Children’s Health Insurance Program (CHIP), and state Basic Health Programs (BHP), although, as discussed further below, those programs use either expected calendar year income or monthly income to determine eligibility, not realized calendar year income.

Tax records have important strengths for the present purposes. The data encompass the entire U.S. population, so they are not subject to sampling error. Additionally, because income reporting is central to tax administration and the relevant reporting processes are very well-established, the income information on tax records is likely of high quality, particularly when the income measure of interest is one used to determine eligibility for tax-based benefits like Marketplace subsidies, as it is here.

By contrast, the reliability of the insurance coverage information reported to the IRS is more uncertain since these reporting requirements are both relatively new and less central to tax administration. Lurie and Pearce examine this question in some detail, but I revisit this question below. In doing so, I focus on the specific tabulations I use in my analysis, tabulations of the number of people with: (1) Marketplace coverage; (2) off-Marketplace nongroup coverage; and (3) no coverage. I consider each in turn.

### *Marketplace Coverage*

The Lurie and Pearce tabulations of Marketplace coverage align very closely with CMS administrative data, which suggests that tax records do a good job of capturing Marketplace coverage. For 2016, Lurie and Pearce report 9.9 million life-years of enrollment in Marketplace coverage, nearly identical to the 10.0 million life-years shown in CMS’ Marketplace effectuated enrollment reports (described further below).

The income distribution of Marketplace coverage reported by Lurie and Pearce also aligns well with estimates of enrollment by income derived from CMS’ Marketplace effectuated enrollment reports and plan selection reports (which are also described further below), as shown in Figure 2. Note that the income distributions in these two data sources would not be expected to agree exactly since the income measure used by Lurie and Pearce reflects actual income for the calendar year, whereas the CMS data generally reflect the income estimate the enrollee provided to the Marketplace at enrollment.<sup>11</sup>

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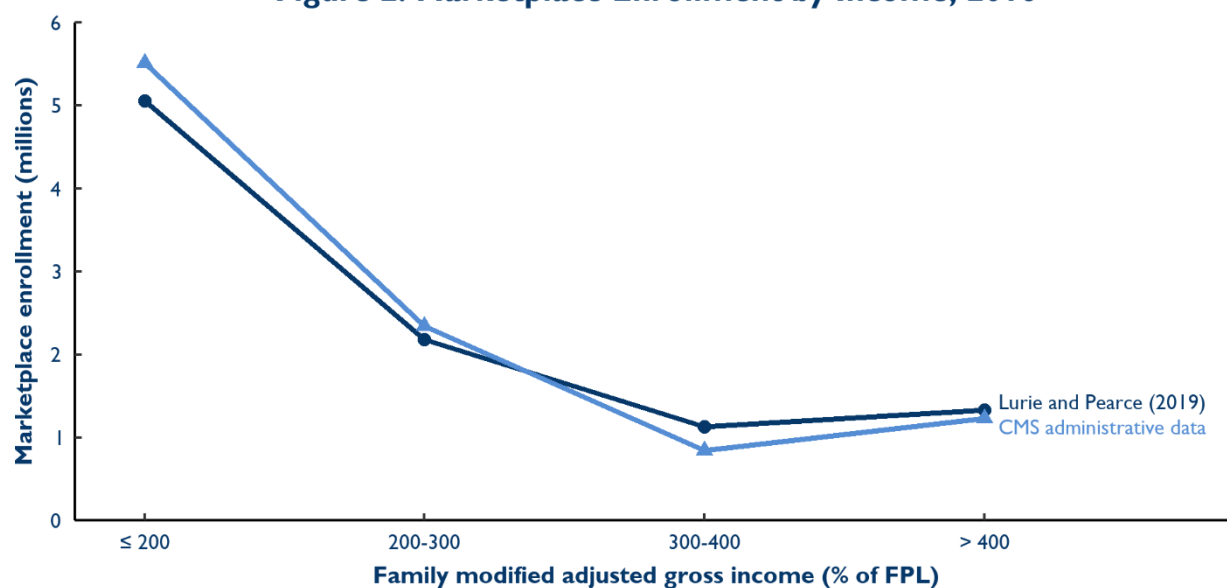
<sup>8</sup> For a full list of what types of coverage constitutes MEC, see 26 C.F.R. §1.5000A-2.

<sup>9</sup> Throughout, I use the term “family” as a briefer alternative to “tax unit.”

<sup>10</sup> I rely on tabulations from the working paper version of the authors’ work since some of the necessary tables are not included in the published version. However, the estimates in the two versions are nearly identical.

<sup>11</sup> Figure 2 obscures one difference between the two data sources. In particular, the plan selection data show very little enrollment among people with incomes below 100% of the FPL, consistent with the fact that such people are

**Figure 2: Marketplace Enrollment by Income, 2016**



Note: Estimates include people under age 65 only. Enrollment measured in life-years.

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### *Off-Marketplace Nongroup Coverage*

Lurie and Pearce’s tabulations of off-Marketplace nongroup coverage also appear broadly consistent with other administrative data sources. However, it is difficult to make an exact comparison because the Lurie and Pearce tabulations encompass both off-Marketplace individual market MEC and “other designated MEC.” The latter category encompasses student health plans, expatriate and inpatriate coverage, group coverage offered to non-employee owners of a business, and certain other forms of coverage.<sup>12</sup>

In detail, Lurie and Pearce report 8.9 million life-years of off-Marketplace nongroup coverage in 2016. For comparison, CMS administrative data (described further below) imply that there were 7.3 million life-years of off-Marketplace individual market MEC (as defined in this paper) in 2016. The difference between these estimates likely reflects the broader scope of the Lurie and Pearce estimates.

### *No Insurance Coverage*

The measures of uninsurance reported by Lurie and Pearce are harder to evaluate. Indeed, Lurie and Pearce use two different methods to estimate the number of uninsured people that generate notably different results. The first counts a person as uninsured if no coverage provider reported providing MEC to that person; it results in an estimate of 52 million life-years of uninsurance in 2016. The second counts

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typically ineligible for subsidized Marketplace coverage. By contrast, the Lurie and Pearce tabulations show substantial enrollment at these income levels. This likely, at least in part, reflects the fact that eligibility for subsidized Marketplace is based on *expected* income for the year, while Lurie and Pearce report tabulations based on *realized* income for the year. I discuss these timing differences in much greater detail below.

<sup>12</sup> The instructions for the 1095-B indicate that certain forms of public coverage, notably Basic Health Program coverage, certain pregnancy-related Medicaid coverage, and Refugee Medical Assistance are also supposed to be reported as other MEC (IRS 2016). However, the authors indicate that these forms of coverage appear to generally be reported as public coverage and, as such, are likely not included in their off-Marketplace category.

a person as uninsured if that person has no third-party-reported MEC *and* does not self-report MEC when filing a tax return; it results in an estimate of just 28 million life-years of uninsurance in 2016.

As discussed by Lurie and Pearce, this difference could arise in three main ways. First, some coverage providers may have failed to report coverage to the IRS. Second, some people with non-MEC policies may have erroneously reported those policies as MEC on their tax returns. Third, some people may have fraudulently reported MEC on their tax returns in an effort to avoid the individual mandate penalty.

For the purposes of this paper, I rely on the Lurie and Pearce estimates that incorporate self-reported coverage information. In doing so, I assume that tax filers are reporting both MEC and non-MEC policies on their tax returns but are not fraudulently reporting MEC in substantial numbers. (A corollary of this assumption is that the Lurie and Pearce uninsurance estimate that does not include self-reported coverage information is higher primarily because some coverage providers are failing to report some MEC, as there is probably not enough enrollment in non-MEC plans to explain the difference between the two metrics. However, the evidence presented above suggests that any underreporting probably predominantly affects forms of MEC other than nongroup MEC.)

This assumption has face plausibility. It seems likely that many tax filers are unable to distinguish MEC and non-MEC policies and so unwittingly report non-MEC policies on their tax returns, and it also seems likely that tax filers shy away from willful fraudulent reporting because they fear penalties for doing so. Comparisons to estimates of the number of uninsured produced using major household surveys also suggest that this assumption is reasonable. The surveys plausibly also capture both MEC and non-MEC policies (since the surveys do not distinguish the two types of policies) and likely do not elicit fraudulent misreports (since there is no incentive to misreport in surveys), and they show similar numbers of uninsured life-years in 2016: 28 million in the National Health Interview Survey, 29 million in the American Community Survey, and 35 million in Medical Expenditure Panel Survey, Household Component.<sup>13,14</sup>

## Description of Supplemental Data Sources

While the Lurie and Pearce estimates provide a rich—and likely highly accurate—picture of coverage status by income group (at least with respect to enrollment in nongroup coverage), they do have limitations. Notably, Lurie and Pearce do not report tabulations for years after 2016, nor can they provide estimates of enrollment in non-MEC nongroup policies (since providers of non-MEC policies are not required to report that coverage to the IRS). The Lurie and Pearce estimates are also not disaggregated by some individual characteristics that are relevant for my analysis, particularly immigration status, state of residence, and whether an individual is eligible for employer coverage. To address these limitations, I supplement the Lurie and Pearce data with other administrative data as well as survey data.

I use two supplemental administrative data sources:

- *Marketplace administrative data:* I use two types of administrative records pertaining to Marketplace enrollment in 2016 and 2019: CMS' annual reports on effectuated Marketplace

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<sup>13</sup> The Current Population Survey Annual Social and Economic Supplement also provides a point-in-time estimate of the number of uninsured as of the time of survey administration in early 2016 of 31 million.

<sup>14</sup> Another concern might, of course, be measurement error in the surveys. While, as discussed further below, there is strong evidence that survey respondents frequently misreport what *type* of coverage they hold, it is plausible that they do a better job reporting whether they hold *any* insurance coverage.

enrollment (the “effectuated enrollment” data) and CMS’ annual reports on the characteristics of people who selected plans during open enrollment (the “plan selections” data).

The effectuated enrollment data report the number of people with active Marketplace coverage in each state and month, but, they do not provide the age or income distribution of Marketplace enrollees. The plan selection data do provide age and income breakdowns, but they include people who never paid premiums to effectuate their coverage and do not include people who enroll outside of open enrollment, so they do not accurately report total enrollment. Thus, I frequently combine the two data sources when estimating Marketplace enrollment.

Specifically, I use the plan selection data to estimate the share of Marketplace enrollment accounted for by each income group and then distribute the aggregate enrollment reported in the effectuated enrollment data according to these shares. In doing so, I must account for a variety of data quirks, particularly the fact that the plan selections data lack income breakdowns in 2016 for states that did not use the HealthCare.gov platform. Appendix A provides full details on my methods. As was shown above in Figure 2, the resulting estimates of Marketplace enrollment by income align well with the estimates reported by Lurie and Pearce for 2016.

- *Medical loss ratio filings:* To administer the ACA’s medical loss ratio (MLR) requirements, CMS requires insurers that offer individual market MEC (including ACA-compliant, grandfathered, and transitional policies) to annually report a wide array of financial and other information, including enrollment.<sup>15</sup> The MLR filings thus provide information on aggregate enrollment in this universe of policies. In combination with the data on Marketplace effectuated enrollment described above, they can also be used to estimate aggregate enrollment in off-Marketplace MEC by subtraction.

I also use four major household surveys: the American Community Survey (ACS); the Current Population Survey Annual Social and Economic Supplement (CPS-ASEC); the Medical Expenditure Panel Survey, Household Component (MEPS-HC); and the Survey of Income and Program Participation (SIPP). All of these surveys provide rich information on the characteristics of household members, including their coverage status, income, demographics, and relationships to one another. Some also provide information on state of residence (ACS, CPS-ASEC, and SIPP) or whether a respondent is offered coverage by an employer (CPS-ASEC and MEPS-HC), a point I discuss at greater length below.

It is frequently useful to place survey respondents into income groups that conceptually align with the income categories reported by Lurie and Pearce. To do so, I use the family relationship and income information to group household members into tax units according to IRS rules. I then calculate each tax unit’s modified adjusted gross income for the 12-month period for which the survey reports that information.<sup>16</sup> Code for assembling tax units and calculating income is available upon request.

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<sup>15</sup> As a technical matter, the MLR reporting requirement, unlike the IRS reporting requirements, is not linked to the definition of MEC, but in practice the MLR definition aligns with the definition of individual market MEC.

<sup>16</sup> In the CPS-ASEC, MEPS-HC, and SIPP, this 12-month period is the calendar year. In the ACS, it is the 12-month period ending with the date on which the respondent answered the survey.

## Limitations of Survey Data Sources

In light of the rich information included in the survey data sources, a natural question is why I do not rely solely on these surveys to produce the estimates in this paper. The most important problem is that the household surveys have well-documented problems measuring nongroup enrollment.

Recent research by Pascale, Fertig, and Call (2019) comparing coverage information collected using the ACS and CPS-ASEC survey instruments to linked administrative records finds that many respondents underreport nongroup coverage (i.e., fail to report nongroup coverage when they do in fact hold it) or overreport nongroup coverage (i.e., report nongroup coverage when they do not in fact hold it). In many cases, this appears to reflect respondents' confusion about what type of coverage they hold. In other cases, respondents may report narrow forms of coverage, like dental insurance, as health insurance. There is little reason to expect other survey data sources to do dramatically better in this regard.<sup>17</sup>

Where there is no alternative to drawing on survey data sources, I take steps to address their limitations whenever feasible. Most importantly, when estimating nongroup enrollment in the ACS and CPS-ASEC, I only count people who report "direct purchase" coverage and no other form of coverage, rather than counting all people who report direct purchase coverage. This approach is motivated by the findings of Pascale, Fertig, and Call (2019). As shown in Appendix B, their results imply that 93% of people who report both direct purchase and another form of coverage in the ACS do not actually hold nongroup coverage as defined in this paper. Excluding these respondents from my measure of nongroup enrollment makes a considerable difference. In the ACS in 2019, for example, excluding these people reduced measured nongroup enrollment by 27% in the non-elderly population and 55% in the full population.<sup>18</sup>

Before proceeding, I note that the survey data sources likely also do a worse job of measuring income than the tax data. This is an important reason for preferring the Lurie and Pearce tabulations of uninsurance to those derived from surveys despite the fact, discussed above, that the Lurie and Pearce tabulations may not measure uninsurance perfectly. Indeed, Lurie and Pearce show a somewhat different income distribution of uninsurance (and the overall population) than the surveys.<sup>19</sup> One corollary of this mismatch is that prior work that has compared nongroup enrollment measured using administrative data to uninsurance measured using survey data could generate biased estimates of take-up rates.

## Methodology

My ultimate goal is to estimate how many life-years non-elderly potential subsidy recipients (as defined in the introduction of this paper) contributed to each of four coverage categories in 2019: (1) Marketplace

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<sup>17</sup> The MEPS-HC is sometimes thought to do somewhat better at measuring what types of coverage people hold (e.g., Hill 2007; Banthin et al. 2019). However, in unreported analysis, I found that the MEPS-HC did a poor job of matching the income distribution of Marketplace enrollment, suggesting misreporting may be a problem there as well. The MEPS-HC also offers smaller sample sizes, particularly relative to the ACS, and it is not yet available for 2019. The MEPS-HC public use file also omits one of the fields needed for this analysis: state of residence.

<sup>18</sup> The larger effect in the full population likely reflects the fact that few elderly people hold nongroup plans as a primary source of coverage, but many do hold Medigap plans and report these as direct purchase coverage.

<sup>19</sup> I experimented with using the ACS to measure uninsurance rather than the Lurie and Pearce data. This would lead to moderately different quantitative estimates but would not change my main qualitative conclusions.

coverage; (2) off-Marketplace MEC; (3) non-MEC nongroup policies; and (4) fully uninsured. I produce estimates for six income groups defined based on calendar year family modified adjusted gross income as a percentage of the FPL: less than 200%, 200-300%, 300-400%, 400-500%, 500-600%, 600% or higher.

To that end, I begin with the Lurie and Pearce tabulations of the number of non-elderly life-years of Marketplace coverage, off-Marketplace MEC, and uninsurance by income group in 2016. I then make a series of adjustments to the Lurie and Pearce estimates.<sup>20</sup> Specifically, I:

- (1) Adjust for the fact that Lurie and Pearce’s estimates of off-Marketplace nongroup MEC enrollment include forms of coverage outside the definition of nongroup coverage used in this paper.
- (2) Derive crude estimates of enrollment in non-MEC nongroup policies in 2016 by income group based on the fragmentary information available on enrollment in those policies.
- (3) Trend the resulting estimates forward to 2019 using a combination of CMS Marketplace administrative data and tabulations of coverage by income in the ACS.
- (4) Adjust the resulting tallies to exclude people who are not potential subsidy recipients using estimates derived from the CPS-ASEC and MEPS-HC.

The remainder of this section describes each step in much greater detail.

### **Step 1: Adjust Lurie and Pearce Off-Exchange Enrollment Estimates**

As described above, Lurie and Pearce’s off-Marketplace nongroup coverage category encompasses both off-Marketplace individual market MEC *and* various other minor coverage types that fall under the heading of “other designated MEC.” Consistent with this, Lurie and Pearce’s estimate of off-Marketplace nongroup enrollment in 2016 is 21% larger than the estimate of off-Marketplace individual market MEC enrollment obtained by comparing the MLR data to CMS’ Marketplace effectuated enrolment reports.

Other designated MEC is outside the scope of this analysis, so I adjust the Lurie and Pearce tabulations to exclude this form of coverage. Unfortunately, I am unaware of any information on the income distribution of other designated MEC. Thus, I make a simple proportional reduction to the off-Marketplace nongroup enrollment reported by Lurie and Pearce for each income group to align their estimates with the aggregate amount of off-Marketplace individual market MEC shown in CMS administrative tallies.<sup>21</sup>

### **Step 2: Estimate Enrollment in Non-MEC Nongroup Policies in 2016**

A major challenge for this analysis is that there is little direct evidence on how many people hold non-MEC nongroup policies. Notably, the Lurie and Pearce estimates do not capture enrollment in non-MEC nongroup policies since insurers are not required to report enrollment in these policies to the IRS.

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<sup>20</sup> Lurie and Pearce note that a small number of people show up in the tax data as having more than one source of coverage. I ignore any overlap in constructing my estimates, as overlap between Marketplace coverage and off-Marketplace MEC seems likely to be particularly rare in practice.

<sup>21</sup> This approach could go seriously awry if there is substantial underreporting of off-Marketplace individual market MEC in the tax data and, thus, “other designated MEC” accounts for a larger fraction of the Lurie and Pearce off-Marketplace nongroup category than it appears. In principle, it should be possible to use the tax data to check whether this is, in fact, the case. Future work along these lines would be worthwhile.

To fill this gap, I proceed as follows. First, I derive a crude estimate of aggregate enrollment in non-MEC nongroup policies in 2019 based on fragmentary information that is in the public domain. Second, I trend that estimate back to 2016 by comparing coverage trends in the ACS (which plausibly captures both MEC and non-MEC policies) to coverage trends in the MLR data (which captures only MEC policies). Third, I assume that enrollment in non-MEC nongroup policies is distributed by age and income in the same way as off-Marketplace MEC. I discuss each of these steps in greater detail below.

### *Derive Crude Estimates of 2019 Enrollment*

I am aware of just two data points on enrollment in non-MEC nongroup policies. First, an investigation by the House Committee on Energy and Commerce that obtained aggregate enrollment information from nine of the largest issuers of short-term limited duration policies found that 3.0 million people purchased such a policy from one of these insurers at some point during 2019 (House Committee on Energy and Commerce 2020). If the typical policy lasts six months (a reasonable, if somewhat arbitrary, assumption), then this would translate to 1.5 million life-years of enrollment in short-term limited duration policies during 2019. Second, the Alliance of Health Care Sharing Ministries (AHCSM), a trade association of health care sharing ministries, reported in May 2020 that 1.0 million people were members of ministries affiliated with the alliance, although it is unclear how this estimate was derived (AHCSM 2020).

Drawing on this evidence, I assume in my primary estimates that there were 3.5 million life-years of enrollment in non-MEC nongroup policies in 2019. This estimate is the sum of the two estimates described in the last paragraph plus an allowance of 1.0 million life-years for enrollment in policies that were sold by companies that are not included in these estimates, as well as enrollment in other categories of non-MEC nongroup policies, particularly fixed indemnity policies. In light of the considerable uncertainty around this estimate, I also present results from sensitivity analyses in which I assume that non-MEC nongroup enrollment in 2019 was 50% higher or 50% lower than this estimate.

### *Trend Estimate Back to 2016 by Comparing ACS and MLR Trends*

I am aware of no data on how enrollment in non-MEC nongroup policies changed from 2016 to 2019.<sup>22</sup> Consequently, I try to infer how enrollment in these non-MEC nongroup policies changed from 2016 to 2019 by comparing enrollment trends in the MLR data to enrollment trends in the ACS.<sup>23</sup>

As described earlier, the MLR data only capture enrollment in nongroup MEC. By contrast, the ACS asks whether each household member was covered by “insurance purchased directly from an insurance company.” While it is unclear how respondents interpret this question, it is plausible that they report both MEC and non-MEC nongroup policies in this category. In principle, therefore, the change in non-MEC nongroup enrollment can be inferred by subtracting the change in nongroup enrollment observed in the MLR data from the change in nongroup enrollment observed in the ACS.

An important complication is that, as noted above, the ACS suffers from both under- and over-reporting of nongroup coverage, which distorts the enrollment trends observed in the ACS. Appendix B presents a

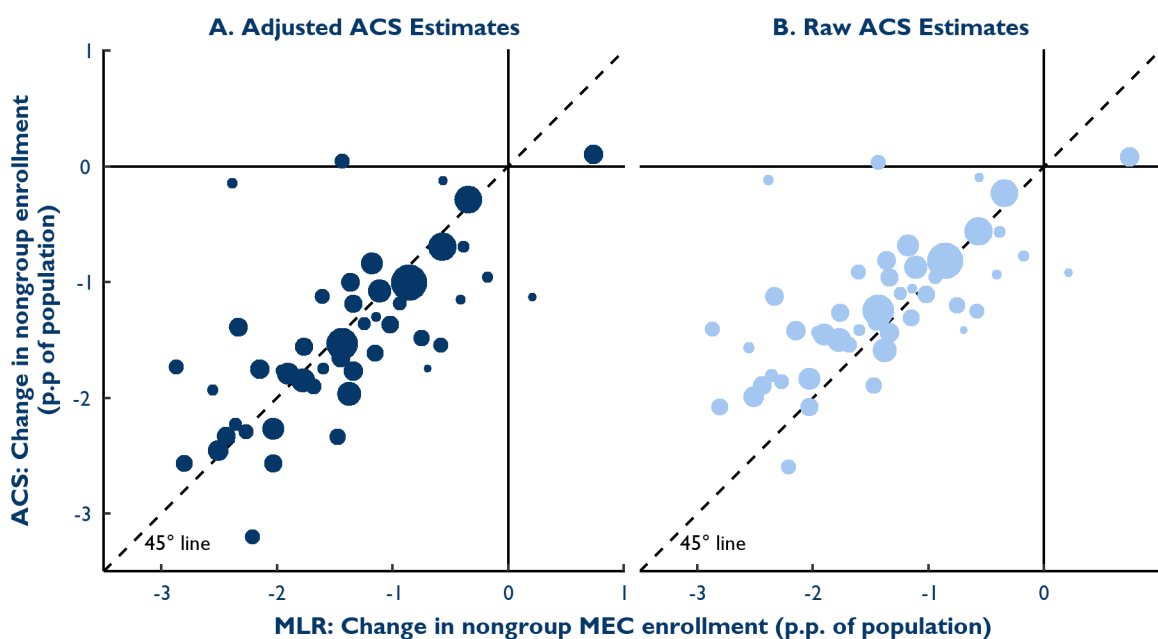
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<sup>22</sup> A partial exception is that archived versions of the AHCSM website report enrollment for earlier periods. In particular, the AHCSM reported enrollment of “more than 600,000” in June 2016 (AHCSM 2016), which suggests that health care sharing ministry enrollment may have risen from 2016 to 2019. However, it is unclear whether these earlier estimates are comparable to the AHCSM’s more recent estimates, and, in any case, enrollment in health care sharing ministries appears to account for a minority of enrollment in non-MEC nongroup policies.

<sup>23</sup> I use the ACS here because its large sample size makes it well suited to produce state-level estimates.



**Figure 3: ACS vs. MLR Nongroup Enrollment Trends by State, 2016-2019**



Note: Marker size reflects state population in 2016. Plot excludes New Hampshire, which experienced an idiosyncratic large decline in MLR-measured enrollment because it stopped covering Medicaid expansion enrollees through nongroup plans during this period. ACS = American Community Survey; MLR = medical loss ratio; MEC = minimum essential coverage.

USC Schaeffer

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simple statistical model of this classification error and derives the usual result (e.g., Aigner 1973) that classification error will tend to attenuate observed changes in nongroup enrollment. In this framework, the degree of attenuation equals the sum of the underreporting rate (i.e., the propensity of people with nongroup coverage to fail to report that coverage) and the overreporting rate (i.e., the propensity of people without nongroup coverage to report such coverage). The relevant underreporting and overreporting rates can then be estimated from the results of Pascale, Fertig, and Call (2019). Those calculations suggest an underreporting rate of 18% and an overreporting rate of 1%. Plugging those estimates into the formula derived in Appendix B then implies that the actual change in nongroup enrollment can be estimated by multiplying the change observed in the ACS by 1.23 ( $=1/(1-0.18-0.01)$ ).

Panel A of Figure 3 displays the resulting comparison between the ACS- and MLR-based estimates of changes in nongroup enrollment at the state level. The two estimated enrollment changes are tightly correlated and very similar on average, which suggests that enrollment in non-MEC nongroup plans changed only slightly over this period. (Panel B shows that the picture would be somewhat different without the adjustment for coverage misreporting. The unadjusted ACS-based estimates typically show smaller nongroup enrollment declines than the MLR-estimates, erroneously suggesting that non-MEC enrollment rose over this period. However, the two measures would still be tightly correlated, reflecting the fact that there is substantial “signal” regarding changes in nongroup enrollment in the ACS data.)

Stepping back to the national level, the MLR data indicate that the share of people holding individual market MEC fell from 5.37% in 2016 to 4.08% in 2019, a decline of 1.29 percentage points. For comparison, after adjusting for coverage misreporting, the ACS indicates that the share of the population with nongroup coverage fell by a slightly larger 1.34 percentage points. Correspondingly, I proceed under the

assumption that the share of the population with non-MEC nongroup policies was 0.05 percentage points larger in 2016 than in 2019. Under my base estimate of non-MEC enrollment in 2019 (which was described above), this implies that 1.1% of the population was enrolled in non-MEC nongroup policies in 2016, corresponding to non-MEC nongroup enrollment of 3.6 million life-years in 2016.<sup>24</sup>

As an aside, I note that the apparent stability of enrollment in non-MEC policies over this period likely reflects the effect of offsetting factors. On the one hand, the relative price of non-MEC nongroup policies fell over this period both because the premiums of ACA-compliant plans rose (e.g., CMS 2019) and because the penalty associated with the ACA's individual mandate (which, with the exception of some health care sharing ministries, non-MEC policies did not satisfy) went away starting in 2019. The Trump administration also substantially liberalized rules governing short-term limited duration plans starting in late 2018. On the other hand, many states implemented new restrictions on short-term plans during this period (Palanker, Kona, and Curran 2019), which likely worked in the opposite direction.

#### *Distribute Across Income Groups Based on Lurie and Pearce Estimates*

I am aware of no evidence on how enrollment in non-MEC nongroup policies is distributed by income. However, much like off-Marketplace MEC, non-MEC nongroup policies likely primarily appeal to people who are not eligible for subsidized Marketplace coverage or who find Marketplace coverage unattractive for some other reason.<sup>25</sup> Correspondingly, in my base estimates, I assume that the income distribution of non-MEC nongroup policies matches the distribution of off-Marketplace MEC enrollment reported by Lurie and Pearce.<sup>26</sup> I also present results from a sensitivity analysis in which I distribute half of non-MEC enrollment proportionally to *all* MEC enrollment, rather than just off-Marketplace MEC enrollment.

### **Step 3: Use ACS and Administrative Data to Trend Estimates Forward to 2019**

Steps 1 and 2 produce estimates for 2016 since the Lurie and Pearce estimates are for that year. But the ultimate goal is to obtain estimates for 2019, so I use a combination of administrative and ACS data to trend the estimates from Steps 1 and 2 forward to 2019. I use different approaches for the Marketplace coverage, off-Marketplace coverage, and uninsured coverage categories. I discuss each in turn.

#### *Marketplace Coverage*

CMS administrative data provide direct information on trends in Marketplace enrollment by income group, so I use these data to trend Marketplace enrollment forward to 2019. To be precise, I begin with the Lurie and Pearce estimate of non-elderly Marketplace enrollment in each income group in 2016. I then add the change in non-elderly Marketplace enrollment in that income group from 2016 and 2019 derived from CMS Marketplace administrative data using the method described in Appendix A.<sup>27</sup>

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<sup>24</sup> In converting this percentage to a number of people, I use Lurie and Pearce's estimate of the total population in 2016, which differs slightly from the ACS estimate, in order to maintain consistency with the rest of the analysis.

<sup>25</sup> The one important difference may be that that relatively healthy people who benefit from the underwriting process are likely to prefer non-MEC policies while others may prefer off-Marketplace MEC policies.

<sup>26</sup> Technically, I assume that the distribution of non-MEC nongroup enrollment by income *and* age matches the distribution of off-Marketplace MEC enrollment reported by Lurie and Pearce. The age portion of this assumption allows me to estimate what (small) fraction of this enrollment is attributable to people over age 65.

<sup>27</sup> One complication is that the estimates derived from the CMS administrative data combine everyone with incomes above 400% of the FPL into a single group. I address this by splitting the change from 2016 to 2019 proportionally to the enrollment shown in the Lurie and Pearce data for each income group in 2016.

### *Off-Marketplace Coverage*

Unlike Marketplace coverage, there are no administrative data that depict trends in off-Marketplace coverage by income group. Thus, I use the ACS to trend *total* nongroup enrollment (i.e., combined enrollment in Marketplace coverage, off-Marketplace MEC, and non-MEC nongroup policies) by income group forward to 2019. I then derive off-Marketplace enrollment as a residual, and I then further decompose this residual into components accounted for by MEC and non-MEC policies.

To be precise, for each income group, I use the estimates from Steps 1 and 2 to calculate the share of the overall non-elderly population that had nongroup coverage and fell in each income group in 2016. I then use the ACS to estimate how the shares for each income group changed from 2016 to 2019; in doing so, I adjust the changes observed in the ACS for coverage misreporting using the method described in Appendix B. I then add the adjusted ACS-derived change to the base 2016 shares and multiply each resulting share by the non-elderly population in 2019 to obtain an estimate of total non-elderly nongroup enrollment in each income group in 2019.<sup>28</sup> (Observe that, by construction, this approach generates an estimate of aggregate nongroup enrollment in 2019 that is consistent with the level of nongroup MEC enrollment shown in the MLR data and the estimate of non-MEC nongroup enrollment derived in Step 2.)

Given these estimates of total nongroup enrollment by income group, I then derive an estimate of off-Marketplace enrollment (including both MEC and non-MEC off-Marketplace policies) for each income group by subtracting Marketplace enrollment. Finally, I split apart MEC and non-MEC off-Marketplace enrollment by assuming that non-MEC enrollment is distributed proportionally to off-Marketplace MEC enrollment, paralleling my approach to distributing non-MEC enrollment in 2016. (Also paralleling my approach above, the sensitivity analyses consider scenarios in which half of non-MEC enrollment is distributed proportionally to total MEC enrollment, rather than just off-Marketplace MEC enrollment.)

### *Uninsured*

There are also no administrative data that depict trends in uninsurance from 2016 to 2019, so I trend the Lurie and Pearce estimates for 2016 forward to 2019 using the ACS. To be precise, I begin with the share of the overall non-elderly population accounted for by non-elderly people who are uninsured and fall in each income group in 2016, which I calculate directly from the Lurie and Pearce estimates. I then use the ACS to estimate how the share corresponding to each income group changed from 2016 to 2019. Finally, I add the ACS-derived change to the base 2016 shares and multiply by the non-elderly population in 2019 to obtain estimates of the number of uninsured by income group in 2019.<sup>29</sup>

### *A Note on Nongroup Coverage Trends*

While the main focus of this paper is on coverage patterns in 2019, I want to pause briefly to remark on the estimated trends in nongroup enrollment by income depicted in Figure 4, which are a byproduct of my method. To my knowledge, estimates of enrollment trends by income group that disaggregate on- and off-Marketplace coverage have not been reported elsewhere.

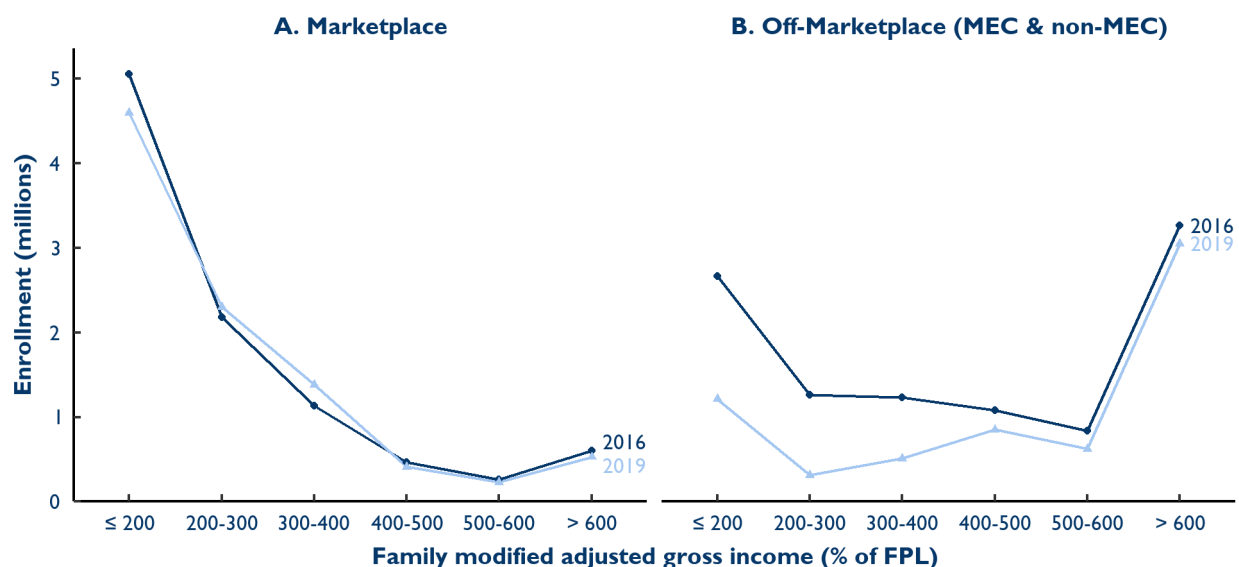
The most striking feature of Figure 4 is the large decline in off-Marketplace enrollment depicted in Panel B. Since I estimate that non-MEC nongroup enrollment was roughly stable over this period, this trend is

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<sup>28</sup> I assume that the total population in 2019 is the total population reported by Lurie and Pearce for 2016, trended forward to 2019 based on the percentage change in the total population shown in the ACS.

<sup>29</sup> I use the same population estimate in this calculation as in the calculation for off-Marketplace coverage above.

**Figure 4: Estimated Nongroup Enrollment, 2016 and 2019**



Note: Estimates include people under age 65 only. Enrollment measured in life-years. MEC = minimum essential coverage.

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driven almost entirely by a decline in off-Marketplace MEC enrollment. This decline has been documented elsewhere (e.g., Fehr, Cox, and Levitt 2019). And the decline itself is not particularly surprising given the large increase in individual market premiums over this period (e.g., CMS 2019). The “churning out” of healthy people who held underwritten grandfathered or transitional policies likely also played a role.

However, to the extent this decline was driven by rising premiums, it is somewhat surprising that the large declines in off-Marketplace enrollment among people with incomes below 400% of the FPL were not matched by increases in *Marketplace* enrollment in that income range. This could indicate that some of these enrollees were unaware that subsidized coverage was available, which would be consistent with survey evidence showing that awareness of the ACA’s Marketplace subsidies is incomplete (Gupta and Collins 2019; Pollitz et al. 2020). Alternatively, it could indicate that the stability of Marketplace enrollment depicted in Panel A may be a bit of an illusion; that is, an influx of enrollees who used to hold off-Marketplace plans may have masked departures from the Marketplace attributable to elimination of the individual mandate penalty, implementation of Medicaid expansion in additional states, and other factors. Thus, this pattern does suggest caution in drawing conclusions about, for example, the effect of repealing the individual mandate based solely on trends in Marketplace enrollment over this period.

Another notable pattern depicted in Figure 4 is that Marketplace enrollment rose among people in the 200-400% of FPL income range even as it fell at other income levels. This pattern may reflect the transition to “silver loading” after the Trump administration ended cost-sharing reduction payments. Silver loading increased the value of the premium and reduced net premiums for bronze and gold plans, which greatly improved enrollees’ options in the 200-400% of the FPL range; it did not similarly improve enrollees’ options at lower income-levels since it did not reduce net premiums for silver plans, and lower-income enrollees are generally best off purchasing silver plans since only silver plans convey eligibility for cost-sharing reductions. Consistent with that, other work has found that silver loading increased enrollment differentially in the 200-400% of FPL group (Aron-Dine 2019; Sprung and Anderson 2018).

## Step 4: Adjust Estimates to Exclude People Outside the Populations of Interest

Steps 1-3 produce estimates of the number of life-years of enrollment in Marketplace coverage, off-Marketplace MEC, and non-MEC nongroup policies, as well as the number of life-years of uninsurance, by income group. The final step is to adjust these estimates to exclude life-years attributable to people who were not potential subsidy recipients, that is, people who: (1) were not legally present in the United States; (2) were eligible for employer or public coverage; or (3) fell in the Medicaid “coverage gap.”

Published administrative tallies do not provide the information needed to estimate how many people fall in these groups.<sup>30</sup> Thus, I use survey data to estimate what share of uninsured and nongroup life-years in each income group are attributable to people outside the population of interest. I then use these shares to proportionally reduce the estimates of enrollment by income group that emerge from Steps 1-3.

In producing the required shares, I rely principally on the CPS-ASEC, as it comes closest to containing all of the needed data elements.<sup>31</sup> I use the 2019 CPS-ASEC despite the fact that the income information and most of the coverage information it collects pertains to 2018 (rather than 2019) since the 2020 CPS-ASEC may have been distorted by the onset of the COVID-19 pandemic.<sup>32</sup> I take different approaches to calculating the excluded shares of uninsured and nongroup life-years, so I describe each in turn.

### *Adjusting Estimates of Uninsured Life-Years*

In calculating the share of uninsured life-years to exclude, the first step is to determine the number of uninsured life-years contributed by each CPS-ASEC respondent. To do so, I use the CPS-ASEC question on calendar year uninsurance, which reports whether the person was uninsured for the full year, part of the year, or none of the year. For these purposes, I treat people who are uninsured for part of the year as contributing 4.5 months of uninsurance, which is the mean number of months of uninsurance among non-elderly part-year uninsured people in the MEPS-HC 2018 Full-Year Consolidated File.<sup>33</sup>

The next step is to identify respondents that have one (or more) of the characteristics that exclude a person from being considered a potential subsidy recipient. The rest of this section discusses, in turn, how I identify respondents that have each of the relevant characteristics. Panel A of Figure 5 summarizes the share of uninsured life-years excluded on the basis of each of these characteristics.

**People not legally present.** The CPS-ASEC directly reports citizenship status, but it does not report whether non-citizens are legally present. Thus, I impute legal status using a method adapted from Borjas (2017), which is in turn a simplified version of the method presented by Passel and Cohn (2014). The

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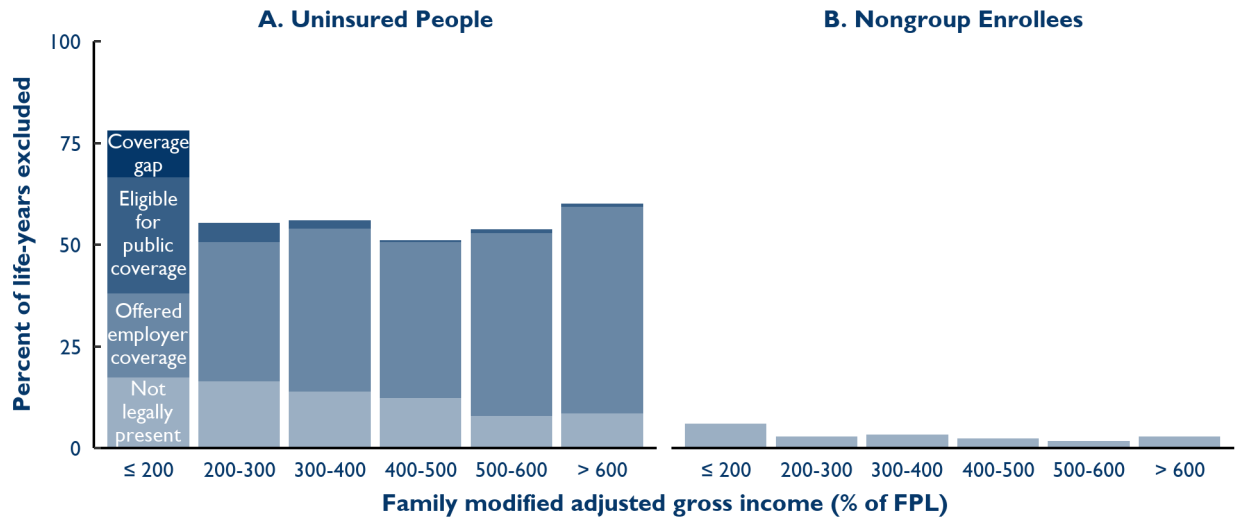
<sup>30</sup> In principle, some of these questions could be answered using the microdata used to produce the Lurie and Pearce estimates. Notably, it would be possible to simulate eligibility for public coverage, and these records would provide some information on eligibility for employer coverage, at least for people employed by large firms.

<sup>31</sup> The ACS and SIPP lack information on employer coverage offers, while the MEPS-HC public use file does not report respondents’ state of residence, which makes it impossible to assess eligibility for public coverage.

<sup>32</sup> The pandemic may have reduced data quality by reducing response rates (Census Bureau 2020). Additionally, if using the 2020 CPS-ASEC, I would have needed to rely on the variable reporting whether the respondent had an employer offer at the time of the survey, which would have been directly affected by the pandemic.

<sup>33</sup> In practice, the large majority of people reporting any uninsurance during 2018 in the CPS-ASEC report being uninsured for the full year, so the precise handling of the part-year uninsured is not particularly important.

**Figure 5: Percent Who Are Not Potential Subsidy Recipients by Income**



Note: Potential subsidy recipients are people who are legally present, ineligible for public and employer coverage, and not in the Medicaid "coverage gap." Each column segment shows the percentage of people excluded from the population of potential subsidy recipients due to the specified characteristic when exclusions are applied from bottom to top. Estimates include people under age 65 only.

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Congressional Budget Office uses a similar method to impute legal status in its health insurance microsimulation model (Banthin et al. 2019). Full details are in Appendix C.

**People eligible for employer coverage:** To determine who has access to employer coverage, I start with the CPS-ASEC question on whether the respondent’s employer offers coverage. The CPS-ASEC question pertains to the time of the interview (February-April 2019) rather than the calendar year period (2018) for which the CPS-ASEC provides income and the uninsurance variable I rely upon. Thus, I use a regression model estimated in the MEPS-HC to “backcast” the employer offer variable; this approach is again similar to the approach used by the Congressional Budget Office (Banthin et al. 2019).

In detail, I use the MEPS-HC Panel 22 Longitudinal File to estimate a logit model in which the outcome is whether the respondent or anyone in the respondent’s family is *currently* offered coverage at work.<sup>34</sup> The predictor variables are an indicator variable for whether the respondent or a family member has an offer early in the subsequent calendar year, as well as a restricted cubic spline in modified adjusted gross income for the current calendar year as a percentage of the FPL. I estimate the model at the person-interview level. I limit the estimation sample to interviews where the respondent is under age 65 and uninsured to match the intended imputation sample as closely as possible.

This regression model produces an estimate of the probability that each respondent had access to employer coverage at any given point in the prior calendar year. For people who otherwise appear to be potential subsidy recipients, I then exclude a share of the associated life-years equal to this probability.<sup>35</sup>

<sup>34</sup> I include family offers since virtually all employers that offer coverage offer dependent coverage (KFF 2020b).

<sup>35</sup> This approach amounts to assuming that the probability a person has an employer offer is uncorrelated with the person’s other characteristics, conditional on the variables included in the prediction model. This assumption is surely not precisely correct, but also may not be a bad approximation.

**People eligible for public coverage.** I impute eligibility for public coverage in two steps. First, I determine whether families are income-eligible for Medicaid, CHIP, or the Basic Health Program based on the calendar year income they report on the CPS-ASEC and state and age-specific income thresholds for Medicaid and CHIP collected by the Kaiser Family Foundation (Brooks, Roygardner, and Artiga 2019), as well as the Basic Health Program eligibility thresholds in the two states that have them (Minnesota and New York).<sup>36</sup> Second, I exclude people who are not legally present, as well as legal immigrants who have held that status for less than five years, as these groups are generally not eligible for Medicaid and CHIP; in doing so, I measure immigration status the same way described above.<sup>37</sup> Due to data limitations, I do not model pregnancy- or disability-related Medicaid and CHIP eligibility pathways, nor do I model eligibility for other forms of public coverage (e.g., Medicare or Veterans Affairs coverage).

A limitation of my approach is that eligibility for public coverage is based on monthly income or, in some cases, expected calendar year income, which will generally differ from realized calendar year income.<sup>38</sup> Consequently, some people with realized calendar year income below the relevant eligibility threshold may not have been eligible for these programs in some months, and vice versa. A similar problem arises if calendar year income is measured with some error in the CPS-ASEC, as seems likely. Appendix D analyzes the potential bias and shows that it is plausibly modest. Consistent with this, when I used the 2018 SIPP (which provides monthly income information) to compare estimates in which I calculated eligibility based on calendar year income to estimates in which I calculated eligibility based on income measures closer to those actually used in eligibility determinations, I found that the income concept used has little effect on the share of uninsured people estimated to be eligible for public coverage.<sup>39</sup>

**People in the “coverage gap.”** The final group I must identify is people who fall in the “coverage gap”; that is, people who are ineligible for Medicaid by virtue of their state’s decision not to expand Medicaid and ineligible for subsidized Marketplace coverage by virtue of having an income below 100% of the FPL. I exclude this group by excluding US citizen respondents who report calendar year income below 100% of the FPL who were not already excluded by virtue of being eligible for public coverage.<sup>40</sup>

As with my approach to imputing eligibility for public coverage, a downside of this approach to identifying people in the coverage gap is that eligibility for subsidized Marketplace coverage is based on expected

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<sup>36</sup> In making these calculations, I apply Medicaid’s definition of a household and its rules for counting income, which differ from those that apply to premium tax credit. Code is available upon request.

<sup>37</sup> An exception is that states do have the option to allow lawfully present non-citizen children and pregnant women to enroll in Medicaid and CHIP even if they have been in the United States for less than five years. I take account of this fact in my eligibility imputations using information on which states have taken up this option collected by the Kaiser Family Foundation (Brooks et al. 2020).

<sup>38</sup> Medicaid/CHIP eligibility determinations are typically based on monthly income. However, for people with monthly income too high to qualify for Medicaid/CHIP but expected calendar year income too low to qualify for subsidized Marketplace coverage (i.e., below 100% of the FPL), Medicaid/CHIP eligibility is instead based on expected calendar year income. For a brief summary of these rules, see CBPP and CLASP (2020).

<sup>39</sup> For the SIPP analyses, I calculated expected calendar year income as the income the person would receive for the calendar year if the individual’s income in each future month matched the current month’s income.

<sup>40</sup> It is necessary to take account of citizenship status in this step because legally present non-citizens are eligible for subsidized Marketplace coverage even if they have income below 100% of the FPL.

calendar year income, which may differ from actual calendar year income; furthermore, income may be measured with error in the CPS-ASEC.<sup>41</sup> As above, however, the analysis in Appendix D suggest that this bias is likely to be modest. Consistent with this, when I used the 2018 SIPP to compare estimates in which eligibility for subsidized Marketplace coverage is calculated based on actual versus expected calendar year income, the share of uninsured life-years estimated to fall in the coverage gap was very similar.

### *Adjusting Estimates of Nongroup Life-Years*

For nongroup coverage, I take a very different approach to excluding life-years belonging to people outside the universe of potential subsidy recipients. In particular, I simply assume that no one who is actually enrolled in nongroup coverage is eligible for public coverage, has access to employer coverage, or falls in the coverage gap. The rationale for this approach is that people in these categories are almost always ineligible for subsidized Marketplace coverage.<sup>42</sup> People with access to public or employer coverage are likely to find that coverage much more attractive than unsubsidized Marketplace coverage since it will almost always offer lower premiums and, in many cases, more comprehensive coverage. And people with incomes low enough to fall in the coverage gap (or to qualify for public coverage) are likely to find unsubsidized Marketplace coverage hard to afford even if they lack other options.

In practice, some people with access to public or employer coverage or who fall in the coverage gap surely do enroll in nongroup coverage. To the extent that is the case, I will overstate the number of nongroup enrollees who are potential subsidy recipients under my definition and, correspondingly, overstate take-up of nongroup coverage in those populations. But this bias seems likely to be relatively small.

Moreover, the alternative of estimating these shares using the CPS-ASEC or other survey seems likely to generate much larger biases for two reasons. First, as discussed at length above, survey respondents commonly report holding nongroup coverage when they in fact hold other forms of coverage (Pascale, Fertig, and Call 2019). Consequently, some people who are eligible for public or employer coverage—and are actually enrolled in public or employer coverage—will report nongroup coverage instead. Because, as described above, people who actually have access to public or employer coverage are very unlikely to enroll in nongroup coverage, this type of misreporting is likely to create a substantial upward bias in estimates of how many nongroup enrollees have access to public or employer coverage.

Second, as also discussed above, surveys measure income with some error, and the calendar year income measure reported on the CPS-ASEC does not precisely correspond to the income measures that govern program eligibility. Appendix D shows that this is likely to create a substantial upward bias in the share of nongroup enrollees estimated to be eligible for public coverage or fall in the coverage gap. Intuitively, this is because some people who are, in fact, eligible for subsidized Marketplace coverage—and actually enroll in that coverage—will *appear* to have lower incomes that either make them eligible for public coverage

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<sup>41</sup> While eligibility for Marketplace subsidies is determined in real time based on expected income, individuals may receive additional premium tax credit or be required to repay some premium tax credit when they file their tax returns if their actual calendar year income ends up differing from their expected income, subject to some limits on how much an individual can be required to repay. See CBPP (2013) for an overview of these rules.

<sup>42</sup> The exception is people offered an employer plan that is not “affordable” (meaning, in 2019, the premium for self-only coverage exceeded 9.86% of income) or that failed to provide “minimum value” (meaning it did not cover at least 60% of expected medical spending).



or put them in the coverage gap. This will substantially distort the estimated shares since people who are *actually* eligible for public coverage or in the coverage gap are unlikely to enroll in nongroup coverage.

I do rely on the CPS-ASEC to estimate the share of non-elderly nongroup enrollees who are not legally present in the United States. As depicted in Panel B of Figure 5, these shares are generally small. In applying these shares, I assume that all nongroup enrollees who are not legally present are enrolled in off-Marketplace coverage since people who are not legally present cannot enroll in Marketplace coverage.

## Results

Figure 6 reports the paper's principal results. The width of each column in Figure 6 represents the share of the overall population of potential subsidy recipients accounted for by each income group, while the height of each segment within a column represents the share of potential subsidy recipients in that income group who have the listed coverage status.<sup>43</sup> These estimates have several notable features.

First, only about half of potential subsidy recipients were enrolled in nongroup MEC, and take-up rates varied only modestly with income; this fraction was 52% among people with incomes below 400% of the FPL, versus 49% above 400% of the FPL. These estimates indicate that there was considerable scope to increase enrollment in nongroup MEC at all income levels, including at income levels where Marketplace subsidies were already available as of 2019. This implies that the recently enacted American Rescue Plan Act, which both made subsidies more generous for people already eligible and extended them above 400% of the FPL, has the potential to increase MEC enrollment across the income distribution.

On its face, it may seem surprising that take-up rates were only weakly related to income despite the fact that higher-income people were ineligible for Marketplace subsidies. Indeed, other evidence shows that insurance enrollment decisions are reasonably price-sensitive (e.g., Hackmann, Kolstad, and Kowalski 2015; Finkelstein, Hendren, and Shepard 2019). However, there are many reasons higher-income people may have a higher willingness to pay for health insurance, such as that they have more assets to protect (e.g., Mahoney 2015). Consistent with this basic story, it is interesting to note that the share of potential subsidy recipients estimated to have any insurance policy (whether an MEC or non-MEC policy) rises sharply with income among people with incomes above 400% of the FPL.

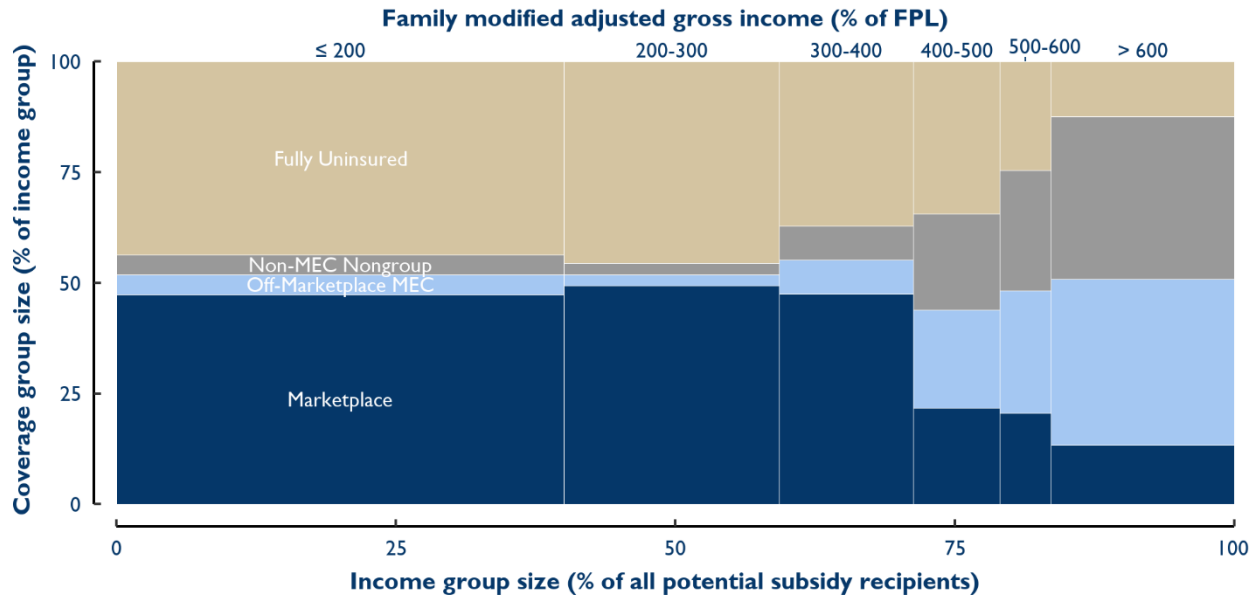
Second, while take-up is incomplete at all income levels, the large majority of potential subsidy recipients who lack nongroup MEC—an estimated 70% of the total—have incomes below 400% of the FPL. This largely reflects the fact that there are relatively few potential subsidy recipients at higher income levels since most people at these income levels have access to employer coverage. This finding implies that not only is there scope to increase enrollment in nongroup MEC among people who were already eligible for subsidies, but most of the opportunity to increase overall take-up is in this group.

Third, the coverage status of potential subsidy recipients who *lack* MEC varies strongly with income. Among potential subsidy recipients without nongroup MEC who had incomes below 400% of FPL, 91% were fully uninsured, while the remaining 9% held non-MEC nongroup policies. By contrast, at higher income levels, 61% held non-MEC nongroup policies and only 39% were fully uninsured. This pattern suggests that increasing MEC enrollment may lead to smaller improvements in financial protection at

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<sup>43</sup> For comparison purposes, Appendix E presents similar figures for two broader populations: (1) all non-elderly people; and (2) all non-elderly people ineligible for public or employer coverage.

**Figure 6: Potential Subsidy Recipients by Income and Coverage, 2019**



Note: Potential subsidy recipients are people who are legally present, ineligible for public and employer coverage, and not in the Medicaid "coverage gap." Estimates include people under age 65 only. Enrollment measured in life-years. MEC = minimum essential coverage.

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higher income levels, although those improvements may still be substantial since non-MEC policies often offer much less robust coverage than MEC policies (e.g., Pollitz et al. 2018; Palanker, Curran, and Salyards 2020). This pattern of enrollment also suggests that efforts to restrict the availability of non-MEC policies would likely have their largest effects on enrollment patterns at higher income levels (whether those effects be to cause people to take up MEC or to forgo insurance entirely).

Fourth, lower-income potential subsidy recipients who held MEC overwhelmingly held Marketplace coverage, while higher-income people generally held off-Marketplace MEC. Among potential subsidy recipients nongroup MEC who had incomes below 400% of the FPL, 91% held Marketplace coverage, whereas just 35% held Marketplace coverage above 400% of the FPL. One implication is that focusing just on Marketplace enrollment can paint a very misleading picture of nongroup enrollment as a whole. This may be particularly important to keep in the mind in tracking the effects of the expansion of the premium tax credit included in the American Rescue Plan Act since one important effect may be to cause people who currently buy off-Marketplace plans to instead obtain coverage inside the Marketplace.

It is also notable that, while Marketplace coverage predominated among people with incomes below 400% of the FPL, there were still some people at these income levels who held off-Marketplace MEC. This could indicate that some enrollees are unaware that subsidized coverage is available, as some surveys suggest (Gupta and Collins 2019; Pollitz et al. 2020). Alternatively, some of these people may be relatively healthy and hold underwritten off-Marketplace policies (grandfathered and transitional MEC policies or non-MEC policies) that are less expensive than subsidized coverage.

Before concluding, I note once again that because of the very limited available data on enrollment in non-MEC nongroup policies, my estimates of the aggregate amount of non-MEC enrollment and how that enrollment is distributed by income are subject to considerable uncertainty. Thus, Appendix F presents the results of sensitivity analyses in which I assume that aggregate enrollment in non-MEC nongroup

policies in 2019 is 50% higher or 50% lower than in my base estimates; the appendix also presents estimates in which I assume that half of non-MEC nongroup enrollment is distributed proportionally to total MEC enrollment instead of all non-MEC nongroup enrollment being distributed proportionally to off-Marketplace MEC enrollment. While these alternative assumptions about non-MEC enrollment lead to modestly different quantitative estimates, my main qualitative conclusions continue to hold.

## Recommendations to Improve Data on Nongroup Enrollment

The goal of this paper is to arrive at the best possible picture of nongroup enrollment given the available data. But if better data were available, it would be possible to produce better estimates and to do so more simply. To that end, I close with two recommendations for improving the available data.

First, and most straightforwardly, the Treasury Department should routinely publish tabulations of insurance coverage by income group based on tax records like those produced by Lurie and Pearce (2019; forthcoming).<sup>44</sup> In future iterations, Treasury could also consider producing tabulations of those data that disaggregate enrollment by state of residence in addition to income. State-level tabulations would make these data much more useful for monitoring state-specific developments and policy changes.<sup>45</sup>

Second, policymakers should create a mechanism for systematically collecting information on enrollment in non-MEC nongroup policies. Indeed, as discussed earlier, the single biggest weakness of my analysis is that I am able to draw on only fragmentary information on enrollment in non-MEC nongroup policies. While non-MEC policies may play a smaller role in the nongroup market if the recent expansion of the ACA's Marketplace subsidies draws enrollees out of non-MEC policies and into the Marketplace, these policies are unlikely to disappear completely, so plugging this data gap is likely to remain valuable.

The best approach to this problem would be to expand the IRS reporting regime that currently applies to insurers that sell MEC policies to encompass non-MEC policies as well. This would be conceptually straightforward to achieve via legislation (albeit politically challenging) and could fit naturally into broader efforts to rationalize regulation of non-ACA-compliant policies (e.g., Linke Young 2020).

But for short-term limited duration policies, it might be possible to extend the IRS coverage reporting regime administratively.<sup>46</sup> In particular, the ACA's implementing regulations exclude short-term limited

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<sup>44</sup> Due to the repeal of the individual mandate penalty effective in 2019, tabulations of the tax data for 2019 and later years would not include information on self-reported MEC; that information was collected from taxpayers as part of the process of assessing individual mandate penalties, which will no longer occur. However, repeal of the individual mandate penalty did not change *coverage providers'* obligations to report MEC to the IRS since those requirements are contained in other provisions of law, so the Lurie and Pearce estimates would continue to provide considerable useful information. The lack of self-reported coverage information might become less important over time as coverage providers gain experience with reporting coverage to the IRS and, correspondingly, the quality of third-party-reported coverage information improves.

<sup>45</sup> It would also be useful to disaggregate the off-Marketplace nongroup MEC category reported by Lurie and Pearce to separate individual market MEC and other designated MEC.

<sup>46</sup> This likely would not be possible for other types of non-MEC policies. Section 26 U.S.C. § 5000A(f)(3) explicitly excludes "excepted benefits" from the definition of MEC, which would likely make it impossible to reach fixed indemnity plans (a form of excepted benefit) through this type of approach. Similarly, 26 U.S.C. § 5000A(d)(2)(B)

duration plans from the definition of individual market MEC—and thus from the IRS’ reporting regime.<sup>47</sup> This exclusion served an important function when the individual mandate was still in effect since it prevented people from satisfying the mandate by purchasing short-term limited duration plans rather than ACA-compliant plans, which would have harmed the risk pool for ACA-compliant policies. But with the mandate penalty now gone, that rationale for defining MEC in this way no longer exists.

Thus, the Treasury Department could consider eliminating the exclusion of short-term limited duration plans from the definition of MEC and thereby making them subject to coverage reporting. Since the definition of MEC is used elsewhere in federal law, it would be important to ensure that this change did not have unintended consequences. Fully exploring those potential ripple effects is beyond the scope of this paper, but I note this change would not affect eligibility for the premium tax credit, which is likely the most important other provision of the ACA linked to the definition of MEC.<sup>48</sup>

If extending the IRS reporting regime is not feasible, it would be valuable to at least collect aggregate information on enrollment in non-MEC policies. The National Association of Insurance Commissioners (NAIC) has taken some steps to collect data on aggregate enrollment in short-term limited duration plans. Notably, it collected these data from many issuers from short-term limited duration plans on a one-time basis in 2019 (NAIC 2021b), and will soon do so on an ongoing basis through the Market Conduct Annual Statement (NAIC 2021a). These data will have limitations, however. They do not encompass other types of non-MEC nongroup policies, such as fixed indemnity policies. And it is unclear whether the data the NAIC collects will ultimately become available to researchers and policymakers (Keith 2020).

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exempts people enrolled in health care sharing ministries from the individual mandate penalty, which suggests that Congress did not intend health care sharing ministries to constitute MEC.

<sup>47</sup> See 26 C.F.R. § 1.5000A-2(d)(1) and 26 U.S.C. § 6055.

<sup>48</sup> In particular, while eligibility for MEC generally prevents a person from claiming the premium tax credit, section 26 U.S.C. § 36B(c)(2)(B)(i) provides an exception for MEC offered in the individual market. Short-term limited duration plans could be considered as such under the approach proposed here.

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## Appendix A: Estimating Marketplace Enrollment by Income

This paper uses CMS' Marketplace effectuated enrollment and plan selections data to derive estimates of non-elderly Marketplace enrollment by income and state. I derive those estimates in three steps:

- (1) I use the plan selections data to calculate the share of plan selections accounted for by each income group in each state and year.
- (2) I distribute effectuated enrollment for each state and year according to those shares.
- (3) I proportionally reduce the state estimates based on the share of plan selections in that state accounted for by people under age 65 and sum across states to obtain national estimates.

The second two steps are straightforward, but the first step is more complex because of two limitations of the CMS data. First, in 2016, CMS only reports the income breakdown of plan selections in states that used the HealthCare.gov platform.<sup>49</sup> Second, the plan selections data use slightly different income categories in the two years.<sup>50</sup> The rest of the appendix describes how I handle both issues.

### States with Missing Income Breakdowns in 2016

To fill in income breakdowns for states where those data are missing in 2016, I first try to locate income breakdowns reported directly by the state Marketplaces. Suitable data are available for California, New York, and Washington State (Covered California 2016; NYSOH 2016; Washington Health Benefit Exchange 2016), which together accounted for 66% of all enrollment in states with missing data.<sup>51</sup>

For the states where neither federal- nor state-reported data are available for 2016, I impute their 2016 Marketplace income distribution by assuming that the change in the Marketplace income distribution in these states mirrored the change in states that have data for both years and have the same Medicaid expansion status. Allowing separate trends by Medicaid expansion status is important since the income distribution of Marketplace enrollment is notably different between Medicaid expansion and non-expansion states (reflecting the fact that people with incomes in the 100-138% of FPL are eligible for subsidized Marketplace coverage in non-expansion states but Medicaid in expansion states).

Formally, I assume that the share plan of selections in state  $s$  and year  $t$  accounted for by an income group  $g \in \{100-150, 150-200, 200-250, 250-300, 300-400, \text{other}\}$  takes the multinomial logit form:

$$p_{sgt} = \frac{\exp(\gamma_{sg} + 1\{t = 2016\}\delta_{M(s)g})}{\sum_k \exp(\gamma_{sk} + 1\{t = 2016\}\delta_{M(s)k})}$$

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<sup>49</sup> Additionally, Idaho does not report the breakdown of plan selections by income in either year, so I assume that the relevant shares match the average in other states that had not adopted Medicaid expansion as of 2019.

<sup>50</sup> Another minor problem is that the data lack income information for enrollees who do not apply for subsidized coverage. In states using the HealthCare.gov platform, these enrollees accounted for 13% of plan selections in 2016 and 14% in 2019. Since the large majority of these enrollees likely have incomes above 400% of the FPL, I treat them all as having income in that range. For states that do not use the HealthCare.gov platform, enrollees not applying for subsidies are already combined with enrollees with incomes above 400% of the FPL.

<sup>51</sup> Some of the income categories used by the state Marketplaces straddle multiple income categories in the federal data. In those cases, I split the state-reported shares proportionally based on the results obtained from the regression imputation procedure described below.

where  $M(s)$  is a categorical variable that indicates whether state  $s$ : (1) adopted Medicaid expansion in January 2014; (2) adopted Medicaid expansion after January 2014 but on or before January 2019; or (3) either adopted Medicaid expansion after January 2019 or never adopted Medicaid expansion. I estimate this model by maximum likelihood and then use the fitted model to impute the missing shares for the states with missing data in 2016.

### Differences in Income Categories Between 2016 and 2019

The second limitation of the CMS data is that they use slightly different income categories in 2016 and 2019. Specifically, in 2016, CMS separately reports the number of Marketplace plan selections by people with incomes below 100% of the FPL and incomes above 400% of the FPL, but in 2019 these two groups are collapsed into a single category. The same problem arises in 2016 for states for which I impute their 2016 income shares based on their 2019 income share. For the states with this problem in 2016, I split the combined category proportionally based on the average size of the underlying categories in 2016 in states in the same Medicaid expansion category (as defined above). In 2019, I split the combined category proportionally based on the (estimated) size of the underlying categories in the same state in 2016.

## Appendix B: Coverage Misreporting in the ACS

This appendix provides additional detail on how I cope with the misreporting of nongroup coverage in the ACS. The first section of the appendix presents calculations based on the results of Pascale, Fertig, and Call (2019) that show that the overwhelming majority of people who report direct purchase coverage in combination with some other form of coverage do not actually hold nongroup coverage. These calculations provide the rationale for estimating nongroup enrollment by only counting people who report direct purchase coverage alone. The second section of the appendix describes the method I use to adjust the coverage trends observed in the ACS for the remaining coverage misreporting.

Before proceeding, I establish some notation. Let  $D$  be a binary random variable indicating whether an individual actually holds nongroup coverage. Additionally, let  $\tilde{D}^O$  be a binary random variable indicating whether a person reports *only* direct purchase coverage in the ACS, and let  $\tilde{D}^A$  be a binary random variable indicating whether a person reports *any* direct purchase coverage, either alone or in combination with some other form of coverage. Due to coverage misreporting,  $D$  need not equal either  $\tilde{D}^A$  or  $\tilde{D}^O$ .

### Effect of Excluding People Who Report Both Direct Purchase and Other Coverage

As described above, I measure nongroup enrollment in the ACS by counting people who report *only* direct purchase coverage (i.e., people with  $\tilde{D}^O = 1$ ) rather than by counting how many report *any* direct purchase coverage (i.e., people with  $\tilde{D}^A = 1$ ). This substantially reduces measured nongroup enrollment, so a key question is what share of the excluded enrollees truly hold nongroup coverage.

I estimate this fraction for the sample of insured people examined by Pascale, Fertig, and Call (2019). Formally, the quantity of interest is  $\mathbb{P}(D = 1 \mid \tilde{D}^A = 1, \tilde{D}^O = 0)$ . This can be rewritten as follows:

$$\mathbb{P}(D = 1 \mid \tilde{D}^A = 1, \tilde{D}^O = 0) = \frac{\mathbb{P}(D = 1 \mid \tilde{D}^A = 1) - \mathbb{P}(D = 1 \mid \tilde{D}^O = 1)\mathbb{P}(\tilde{D}^O = 1 \mid \tilde{D}^A = 1)}{1 - \mathbb{P}(\tilde{D}^O = 1 \mid \tilde{D}^A = 1)}.$$

Two of the probabilities on the right-hand side of this equation are directly reported by Pascale, Fertig, and Call (2019) for their validation sample (see Table 5 or Table 6). The exception is  $\mathbb{P}(\tilde{D}^O = 1 \mid \tilde{D}^A = 1)$ ,

the share of people who report any direct purchase coverage who report *only* direct purchase coverage. I estimate this share using the 2015 ACS, limiting the sample to people under age 65 in Minnesota who do not report Medicare coverage in order to match the Pascale, Fertig, and Call sample frame as closely as possible. This yields an estimate  $\mathbb{P}(\tilde{D}^O = 1 \mid \tilde{D}^A = 1) = 0.785$ .

Plugging these probabilities into the equation above yields  $\mathbb{P}(D = 1 \mid \tilde{D}^A = 1, \tilde{D}^O = 0) = 0.07$ . That is, only 7% of people who report direct purchase coverage in combination with another form of coverage actually held nongroup coverage. In light of the composition of the Pascale, Fertig, and Call sample, this estimate is likely a reasonable guide to the effect of moving from  $\tilde{D}^A$  to  $\tilde{D}^O$  when generating estimates for the non-elderly population in the ACS writ large. For the full population (including the elderly), this may even overestimate the share that actually hold nongroup coverage since very few elderly people actually hold nongroup coverage, but many likely report Medigap plans as direct purchase coverage.

### Adjusting ACS Nongroup Coverage Trends for Misreporting

I now describe how I adjust raw trends measured using the ACS to remove distortions due to misreporting. I first describe my methodology for adjusting changes in the overall share of the population with nongroup coverage, and I then turn to changes in the share of the population that has nongroup population *and* falls in a particular income group. Those discussions show that adjusting for distortions created by misreporting requires knowing the rate at which respondents under- and over-report nongroup coverage, so I conclude this section by using the results of Pascale, Fertig, and Call (2019) to estimate those rates.

#### Overall Coverage Shares

I first consider how to adjust changes in the overall share of the population with nongroup coverage for misreporting. To do so, I first assume that the propensity to under- and over-report coverage is constant over time. Then, letting  $\mathbb{P}_t$  be the probability measure corresponding to survey samples taken time  $t$ , I can define the following parameters that are constant across all times  $t$ :

$$\alpha \equiv \mathbb{P}_t(\tilde{D}^O = 1 \mid D = 0) \text{ and } \beta \equiv \mathbb{P}_t(\tilde{D}^O = 0 \mid D = 1),$$

where  $\alpha$  is the propensity of a person without nongroup coverage to erroneously report that they do have direct purchase coverage (i.e., over-report), while  $\beta$  is the propensity of a person with nongroup coverage to erroneously report that they do not have direct purchase coverage (i.e., under-report).

The share of people observed to have nongroup coverage at time  $t$  can then be written as

$$\mathbb{P}_t(\tilde{D}^O = 1) = \alpha \mathbb{P}_t(D = 0) + (1 - \beta) \mathbb{P}_t(D = 1),$$

from which it follows that the observed change in that share from a time  $t_0$  to a time  $t_1$  is

$$\mathbb{P}_{t_1}(\tilde{D}^O = 1) - \mathbb{P}_{t_0}(\tilde{D}^O = 1) = (1 - \beta - \alpha) [\mathbb{P}_{t_1}(D = 1) - \mathbb{P}_{t_0}(D = 1)].$$

That is, the change in the share of the population observed to have nongroup coverage is the actual change in the share with nongroup coverage discounted by a factor  $1 - \beta - \alpha$ ; this is a standard result from the literature on classification error (e.g., Aigner 1973).<sup>52</sup> It is then easy to see that, given  $\alpha$  and  $\beta$ , the actual change can be recovered by dividing the observed change by this factor.

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<sup>52</sup> In theory, it could be the case that  $\alpha > 1 - \beta$  in which case the observed change would actually have the opposite sign from the actual change. This is clearly not the case in practice.

### Coverage-by-Income Shares

Next, I consider how to adjust changes in the share of the population observed to hold direct purchase coverage *and* fall in a specific income group. To that end, I let  $Y$  be a random variable denoting a respondent's income. I again assume that the propensity to misreport coverage is constant over time, and I define income-group-specific misreporting rates

$$\alpha_k \equiv \mathbb{P}_t(\tilde{D}^O = 1 \mid D = 0, Y = k) \quad \text{and} \quad \beta_k \equiv \mathbb{P}_t(\tilde{D}^O = 0 \mid D = 1, Y = k),$$

which are directly analogous to the misreporting rates  $\alpha$  and  $\beta$  defined above.

The share of people who are observed to have nongroup coverage and fall in income group  $k$  at time  $t$  is

$$\mathbb{P}_t(\tilde{D}^O = 1, Y = k) = \alpha_k \mathbb{P}_t(D = 0, Y = k) + (1 - \beta_k) \mathbb{P}_t(D = 1, Y = k),$$

from which it follows that the observed change in this share from a time  $t_0$  to a time  $t_1$  is

$$\begin{aligned} & \mathbb{P}_{t_1}(\tilde{D}^O = 1, Y = k) - \mathbb{P}_{t_0}(\tilde{D}^O = 1, Y = k) \\ &= \alpha_k [\mathbb{P}_{t_1}(Y = k) - \mathbb{P}_{t_0}(Y = k)] + (1 - \beta_k - \alpha_k) [\mathbb{P}_{t_1}(D = 1, Y = k) - \mathbb{P}_{t_0}(D = 1, Y = k)]. \end{aligned}$$

The true change of interest can thus be calculated as

$$\begin{aligned} & \mathbb{P}_{t_1}(D = 1, Y = k) - \mathbb{P}_{t_0}(D = 1, Y = k) \\ &= \frac{1}{1 - \beta_k - \alpha_k} [\mathbb{P}_{t_1}(\tilde{D}^O = 1, Y = k) - \mathbb{P}_{t_0}(\tilde{D}^O = 1, Y = k) - \alpha_k \{\mathbb{P}_{t_1}(Y = k) - \mathbb{P}_{t_0}(Y = k)\}]. \end{aligned}$$

Relative to the case where the object of interest was the change in the overall share of the population with nongroup coverage, the true change is now a more complicated function of the observed change and now depends on the change in the share of the population that falls in that income group. But knowledge of the misreporting parameters  $\alpha_k$  and  $\beta_k$  remains sufficient to recover the true change.

### Calibrating the Under- and Over-reporting Rates

I estimate the misreporting rates using results reported by Pascale, Fertig, and Call (2019). The Pascale, Fertig, and Call results are not disaggregated by income, so I proceed under the assumption that misreporting rates are constant across income groups and, correspondingly, estimate a single  $\alpha$  and  $\beta$  that applies to all income groups. Similarly, I assume that the values of  $\alpha$  and  $\beta$  I obtain for the Pascale, Fertig, and Call sample frame are generalizable to the full US population.

In reality,  $\alpha$  and  $\beta$  surely vary somewhat across population groups. However,  $\alpha$  can vary only over a relatively small range since it is bounded below by zero and bounded above by the share of the population that reports nongroup coverage; in practice, varying  $\alpha$  over this range has little effect on the results. It is plausible that  $\beta$  also varies relatively little since it is not immediately clear what would spur people who hold nongroup coverage in different population groups to report that coverage differently. Thus, the assumption that  $\alpha$  and  $\beta$  are constant across the population seem like a reasonable one.

Most of the estimates reported by Pascale, Fertig, and Call concern respondents' propensity to report holding *any* direct purchase coverage (that is,  $\tilde{D}^A$ ), whereas I focus on individuals' propensity to report *only* direct purchase coverage (that is,  $\tilde{D}^O$ ). Thus, deriving suitable estimates of  $\alpha$  and  $\beta$  requires making some calculations based on their results. I begin with  $\beta$ . Observe that

$$\beta = \mathbb{P}(\tilde{D}^A = 0 \mid D = 1) + \mathbb{P}(\tilde{D}^A = 1, \tilde{D}^O = 0 \mid D = 1),$$

where I use  $\mathbb{P}$  to denote the probability measure corresponding to the authors' sample frame.

Pascale, Fertig, and Call estimate that the first term on the right-hand side of this equation, the share of nongroup enrollees that completely fail to report direct purchase coverage under the ACS survey instrument, is 0.15 (see Table 2). To estimate the second term, the share of nongroup enrollees who report direct purchase coverage in combination with some other form of coverage, observe that

$$\begin{aligned} & \mathbb{P}(\tilde{D}^A = 1, \tilde{D}^O = 0 \mid D = 1) \\ &= [\mathbb{P}(D = 1 \mid \tilde{D}^A = 1) - \mathbb{P}(D = 1 \mid \tilde{D}^O = 1)] \mathbb{P}(\tilde{D}^O = 1 \mid \tilde{D}^A = 1) \frac{\mathbb{P}(\tilde{D}^A = 1)}{\mathbb{P}(\tilde{D}^O = 1)}. \end{aligned}$$

All of the probabilities on the right-hand-side of this equation are directly reported by Pascale, Fertig, and Call (2019)—variously in Tables 2, 5, and 6—except for  $\mathbb{P}(\tilde{D}^O = 1 \mid \tilde{D}^A = 1)$ , the share of people who report any direct purchase coverage who report *only* direct purchase coverage. I estimate this probability using the 2015 ACS using the same method used earlier in this appendix. Plugging the relevant probabilities into this equation implies that an additional 2.6% of nongroup enrollees report direct purchase coverage in combination with some other form of coverage, so I obtain  $\beta = 0.176$ .

I now turn to  $\alpha$ . Observe that

$$\alpha = \mathbb{P}(D = 0 \mid \tilde{D}^O = 1) \mathbb{P}(\tilde{D}^O = 1 \mid \tilde{D}^A = 1) \frac{\mathbb{P}(\tilde{D}^A = 1)}{\mathbb{P}(D = 0)}.$$

As above, all of the probabilities in the above equation are reported directly by Pascale, Fertig, and Call (in Table 2 or Table 6) except for  $\mathbb{P}(\tilde{D}^O = 1 \mid \tilde{D}^A = 1)$ , which I once again estimate using the same method as earlier in this appendix. Plugging into the equation above yields  $\alpha = 0.014$ .

## Appendix C: Methodology for Imputing Immigration Status

To identify unauthorized immigrants in the survey data, I start with the algorithm used by Borjas (2017), which is a simplified version of the method presented by Passel and Cohn (2014). I then slightly modify the Borjas algorithm, mainly to incorporate features of the algorithm of Passel and Cohn (2018) that appear likely to improve the accuracy of the results. The Congressional Budget Office has also used a methodology based on Borjas' method to identify unauthorized immigrants in survey data when constructing its health insurance coverage microsimulation model (Banthin et al. 2019).

In detail, I treat a person as being legally present in the United States if the person reports having one of the following characteristics that implies legal status with certainty or with high probability:

- Being a U.S. citizen (unless the person reports being a naturalized citizen who is from Mexico or Central America, arrived in the United States in the last three years, or arrived in the United States in the last six years and does not have a citizen spouse);<sup>53</sup>

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<sup>53</sup> The CPS ASEC does not report respondents' exact year of arrival in the United States. For this reason, I treat people as having arrived in the last six years if they arrived in 2014 or later (which encompasses slightly more than five years

- Having arrived in the United States prior to 1980;
- Being enrolled in one of: Medicaid/CHIP; Medicare; Marketplace coverage; health care through the Department of Defense, TRICARE, or Department of Veterans Affairs; public housing; veterans' benefits; unemployment compensation; Supplemental Security Income; Social Security; Supplemental Nutrition Assistance Program; or any form of rental or cash assistance;<sup>54</sup>
- Being a current member of the armed forces or a veteran of the armed forces;
- Being employed by a federal, state, or local government or in one of the following occupational categories: legal occupations; health care practitioners and technical occupations; and protective service occupations;
- Having been born in Cuba; or
- Being the spouse or child of a person that meets one of the above criteria.

My method differs from Borjas' method in a few respects. First, Borjas treats anyone who reports U.S. citizenship as being legally present. However, Passel and Cohn (2018) summarize evidence that people who recently arrived in the United States or who immigrated from Central America or Mexico often overreport naturalized citizenship in surveys. Thus, following Passel and Cohn, I do not automatically assign legal status to people in these groups who report naturalized citizenship. Second, Borjas assigns legal status to spouses of people who have another marker of legal status, but I do so for both spouses and children; in doing so, I again conform somewhat more closely to the approach used by Passel and Cohn (2018).<sup>55</sup> Third, relative to Borjas, I assign legal status based on participation in a somewhat broader list of public programs and a slightly different list of occupations.

Even with the modifications described above, the method I use here still differs in important respects from the more complex approach of Passel and Cohn (2018). Notably, Passel and Cohn (2018) start by assigning legal status to survey respondents based on criteria similar to (but more complex than) the criteria used here. They then probabilistically assign unauthorized status to the remaining respondents to align with estimates of the aggregate size of the undocumented population (which are themselves derived by subtracting administrative tallies of the number of lawful immigrants from the number of foreign-born individuals estimated to be present in the United States using survey data).

However, Borjas shows that his method generates very similar legal status assignments—both in the aggregate and at the individual level—to the method of Passel and Cohn (2014), which is in turn very

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prior to the interview). I treat them as having arrived in the last three years if they arrived in 2016 or later (which, similarly, encompasses slightly more than the three years prior to the interview).

<sup>54</sup> Participation in housing programs as well as the Supplemental Nutrition Assistance Program is only observed at the household level, so I treat a person as participating in these programs if the person lives in a household in which at least one person participates.

<sup>55</sup> On the other hand, there is one respect in which I remain closer to Borjas. Passel and Cohn (2018) generally do not impute legal status to relatives of legal permanent residents, whereas both Borjas and I implicitly do.

similar to the updated method of Passel and Cohn (2018). It thus seems likely that the legal status assignments generated by my modified version of Borjas' method would be similar to those obtained from the method of Passel and Cohn (2018). Indeed, when applied in the 2019 CPS-ASEC, my methodology estimates that there were 11.2 million people who are not legally present in early 2019. For comparison, Passel and Cohn (2019) estimate there were 10.5 million unauthorized immigrants as of 2017.

Some other work on health insurance enrollment (e.g., SHADAC 2013; Capps et al. 2013; Garfield, Orgera, and Damico 2020) has statistically imputed immigration status using the Survey of Income and Program Participation (SIPP). This approach takes advantage of the fact that some older SIPP vintages asked non-citizens whether they were legal permanent residents at the time of interview. That makes it possible to develop a statistical model for predicting lawful status based on individual characteristics, which can then be used to impute lawful status in other survey datasets. In some analyses (e.g., SHADAC 2013; Capps et al. 2013), this type of imputation model is the sole method used to identify unauthorized immigrants. In other cases (e.g., Garfield, Orgera, and Damico 2020), the SIPP-derived imputation model is used in combination with aggregate targets derived through a method like that of Passel and Cohn (2018).

While the SIPP-based approach is quite appealing in concept, the SIPP ceased asking about current immigration status after the 2008 SIPP panel. Thus, the most recent SIPP data suitable for this type of approach are now more than a decade old, and it is thus unclear whether (and, if so, how much) imputing immigration status based on the SIPP would improve over the method used here.

## Appendix D: Consequences of Imperfect Income Measurement

This paper uses survey data to estimate what share of people with a particular coverage status (variously, either uninsured or enrolled in nongroup coverage) are eligible for some form of subsidized coverage (variously, either subsidized Marketplace coverage or public coverage). As described in the main text, a limitation of my approach is that the full-year income measure reported on the CPS-ASEC differs from the income measures used to determine program eligibility (monthly income, expected annual income, or some combination of the two), and income is likely measured with some error in the CPS-ASEC. This appendix analyzes how these imperfections in income measurement might bias my results.

### General Setup

Formally, I let  $Y_O$  denote the full-year income measure observed in the CPS-ASEC,  $Y_F$  the respondent's actual full-year income, and  $Y_P$  the income measure used to determine eligibility for the relevant program. I let  $E(y)$  denote the event that the person would be eligible for the program in question with income  $y$ .<sup>56</sup> For convenience, I then define events  $E_O = E(Y_O)$  and  $E_P = E(Y_P)$ , which capture whether the respondent would be eligible based on each of the income measures  $Y_O$  and  $Y_P$ . I use  $N_j = E_j^c$  for each  $j \in \{O, P\}$  to denote the complementary events where the person is ineligible for the program of interest based on each income measure. Finally, I let  $T$  denote the event in which the respondent has the coverage status of interest (either uninsured or enrolled in nongroup coverage).

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<sup>56</sup> The values taken by the family of random variables  $E(y)$  will generally differ across respondents because it depends on a respondent's other characteristics, such as the respondent's state of residence.

In this notation, my ideal estimand is  $\mathbb{P}(E_P|T, Y_F \in \mathcal{A})$ , where the set  $\mathcal{A}$  defines the income group being examined. However, because only  $Y_O$  (and not  $Y_P$  and  $Y_F$ ) are observed, I instead obtain the alternative estimand  $\mathbb{P}(E_O | T, Y_O \in \mathcal{A})$ . The question is how these estimands compare. To that end, observe that

$$\mathbb{P}(E_O|T, Y_O \in \mathcal{A}) = \mathbb{P}(E_P|T, Y_F \in \mathcal{A}) + \underbrace{\mathbb{P}(E_P|T, Y_O \in \mathcal{A}) - \mathbb{P}(E_P|T, Y_F \in \mathcal{A})}_{\text{bias from averaging over the wrong population}} + \underbrace{\mathbb{P}(E_O|T, Y_O \in \mathcal{A}) - \mathbb{P}(E_P|T, Y_O \in \mathcal{A})}_{\text{bias from mismeasuring eligibility}}. \quad (*)$$

This equation demonstrates that the actual estimand equals the ideal estimand, plus two bias terms. The first bias term reflects the fact that the actual estimand averages over the wrong population: respondents with  $Y_O \in \mathcal{A}$  rather than respondents with  $Y_F \in \mathcal{A}$ . The second bias term reflects the fact that the actual estimand is based on the wrong eligibility measure:  $E_O$  rather than  $E_P$ .

### Bias from Averaging Over the Wrong Population

To assess the magnitude of the first bias term, observe that it can be written in the form

$$\begin{aligned} & \mathbb{P}(E_P|T, Y_O \in \mathcal{A}) - \mathbb{P}(E_P|T, Y_F \in \mathcal{A}) \\ &= \mathbb{P}(E_P|T, Y_F \in \mathcal{A}, Y_O \in \mathcal{A}) \frac{\mathbb{P}(Y_O \in \mathcal{A}, Y_F \in \mathcal{A}|T)}{\mathbb{P}(Y_O \in \mathcal{A}|T)\mathbb{P}(Y_F \in \mathcal{A}|T)} [\mathbb{P}(Y_F \in \mathcal{A}|T) - \mathbb{P}(Y_O \in \mathcal{A}|T)] \\ &+ \mathbb{P}(E_P|T, Y_F \notin \mathcal{A}, Y_O \in \mathcal{A}) \frac{\mathbb{P}(Y_F \notin \mathcal{A}, Y_O \in \mathcal{A}|T)}{\mathbb{P}(Y_O \in \mathcal{A}|T)} \\ &- \mathbb{P}(E_P|T, Y_F \in \mathcal{A}, Y_O \notin \mathcal{A}) \frac{\mathbb{P}(Y_F \in \mathcal{A}, Y_O \notin \mathcal{A}|T)}{\mathbb{P}(Y_F \in \mathcal{A}|T)} \end{aligned}$$

This equation implies that this bias term will be small if: (1) the probability that a person is erroneously included in the sample is similar to the probability that a person is erroneously excluded, that is,  $\mathbb{P}(Y_F \notin \mathcal{A}, Y_O \in \mathcal{A}|T) \approx \mathbb{P}(Y_F \in \mathcal{A}, Y_O \notin \mathcal{A}|T)$  or, equivalently,  $\mathbb{P}(Y_F \in \mathcal{A}|T) \approx \mathbb{P}(Y_O \in \mathcal{A}|T)$ ; and (2) the people erroneously included and erroneously excluded are eligible for the program of interest at similar rates, that is, if  $\mathbb{P}(E_P|T, Y_F \notin \mathcal{A}, Y_O \in \mathcal{A}) \approx \mathbb{P}(E_P|T, Y_F \in \mathcal{A}, Y_O \notin \mathcal{A})$ . Under these conditions, the first term roughly vanishes, and the last two terms offset each other.

Both of these conditions are likely to hold as long as: the boundary of the set  $\mathcal{A}$  is not close to any important eligibility thresholds; and the distribution of the measurement error  $Y_O - Y_F$  conditional on  $Y_F$  is reasonably symmetric, not too large, and not too dependent the actual value of  $Y_F$ . In particular, if the boundary is not near any important eligibility thresholds, then it is likely that  $\mathbb{P}(E_P|T, Y_F \notin \mathcal{A}, Y_O \in \mathcal{A}) \approx \mathbb{P}(E_P|T, Y_F \in \mathcal{A}, Y_O \notin \mathcal{A})$ , and, furthermore, the density of  $Y_F|T$  will not be too steeply sloped near the boundary of  $\mathcal{A}$ . This latter fact, together with the properties of the measurement error described above will tend to result in  $\mathbb{P}(Y_F \notin \mathcal{A}, Y_O \in \mathcal{A}|T) \approx \mathbb{P}(Y_F \in \mathcal{A}, Y_O \notin \mathcal{A}|T)$ . These assumptions are plausible in my context, so it follows that the first bias term can likely be neglected for my purposes.

### Bias from Mismeasuring Eligibility

I now turn to the second bias term in equation (\*), which captures the bias from mismeasuring eligibility. To assess the likely magnitude of the second bias term, observe that it can be rewritten as follows



$$\begin{aligned} & \mathbb{P}(E_O|T, Y_O \in \mathcal{A}) - \mathbb{P}(E_P|T, Y_O \in \mathcal{A}) \\ &= \frac{\mathbb{P}(T|E_O, N_P, Y_O \in \mathcal{A})}{\mathbb{P}(T | Y_O \in \mathcal{A})} \mathbb{P}(E_O, N_P|Y_O \in \mathcal{A}) - \frac{\mathbb{P}(T|N_O, E_P, Y_O \in \mathcal{A})}{\mathbb{P}(T | Y_O \in \mathcal{A})} \mathbb{P}(N_O, E_P|Y_O \in \mathcal{A}). \end{aligned}$$

The equation illustrates that this bias term will be small if: (1) the probability that a person is erroneously classified as eligible for the program in question is similar to the probability that the person is erroneously classified as ineligible, that is,  $\mathbb{P}(E_O, N_P|Y_O \in \mathcal{A}) \approx \mathbb{P}(N_O, E_P|Y_O \in \mathcal{A})$ ; and (2) the coverage outcomes of people who are erroneously categorized as eligible are similar to those of people who are erroneously categorized as ineligible, that is,  $\mathbb{P}(T|E_O, N_P, Y_O \in \mathcal{A}) \approx \mathbb{P}(T|N_O, E_P, Y_O \in \mathcal{A})$ . Under these conditions, the two terms in the equation above will approximately offset each other.

The first of these conditions seems likely to hold as long as: the relevant eligibility threshold is not close to the boundary of  $\mathcal{A}$ ; and the distribution of the error  $Y_O - Y_P$  conditional on  $Y_P$  is reasonably symmetric, not too large, and not too dependent on the actual value of  $Y_P$ . These assumptions are reasonable here.

The second condition clearly will not hold when the outcome of interest is nongroup enrollment. This is likely to be true regardless of whether the focus is estimating the share of nongroup enrollees eligible for public coverage or estimating the share of nongroup enrollees who fall in the coverage gap:

- *Share of nongroup enrollees eligible for public coverage:* Before proceeding, observe that, in this case, the events  $E_j$  and  $N_j$  should be taken to correspond to eligibility for *public* coverage. People eligible for public coverage are ineligible for subsidized Marketplace coverage, so very few people in the relevant income range are likely to opt for unsubsidized coverage over public coverage. Hence,  $\mathbb{P}(T|N_O, E_P, Y_O \in \mathcal{A}) \approx 0$ . Furthermore, the adding-up constraint then implies that  $\mathbb{P}(T|E_O, N_P, Y_O \in \mathcal{A})$  is likely to be similar to or greater than  $\mathbb{P}(T | Y_O \in \mathcal{A})$ . It follows that the second bias term may be strongly positive, and the share of nongroup enrollees estimated to be eligible for public coverage may be far larger than the true share.
- *Share of nongroup enrollees in the coverage gap:* In this case, the events  $E_j$  and  $N_j$  should be taken to correspond to eligibility for *Marketplace* coverage. People who fall in the coverage gap are, by definition, ineligible for subsidized Marketplace coverage, so few are likely to enroll in nongroup coverage. Hence,  $\mathbb{P}(T|E_O, N_P, Y_O \in \mathcal{A}) \approx 0$ . Furthermore, the adding up constraint then implies that  $\mathbb{P}(T|N_O, E_P, Y_O \in \mathcal{A})$  is likely to be greater than  $\mathbb{P}(T | Y_O \in \mathcal{A})$ . It follows that the second bias term may be strongly negative, so the share of nongroup enrollees in this income range estimated to be eligible for subsidized Marketplace coverage may be far smaller than the true share, and, correspondingly, the share estimated to fall in the coverage gap may be far larger.

By contrast, the second condition above may be more reasonable when the outcome of interest is uninsurance. Once again, this is true regardless of whether the focus is estimating the share of uninsured people who are eligible for public coverage or the share who fall in the coverage gap:

- *Share of uninsured enrollees eligible for public coverage:* In this case, the events  $E_j$  and  $N_j$  should once again be taken to correspond to eligibility for *public* coverage. For almost all of the public coverage eligibility thresholds of interest in this paper, a person is eligible for *some* form of subsidized coverage regardless of what side of the eligibility threshold the individual falls on. Moreover, take-up of Medicaid is known to be incomplete, and the estimates in this paper

demonstrate that the same is true of subsidized Marketplace coverage. That implies that the difference between  $\mathbb{P}(T|E_0, N_p, Y_0 \in \mathcal{A})$  and  $\mathbb{P}(T|N_0, E_p, Y_0 \in \mathcal{A})$  must be much smaller than in the nongroup enrollment case, suggesting that bias is likely to be much smaller as well.

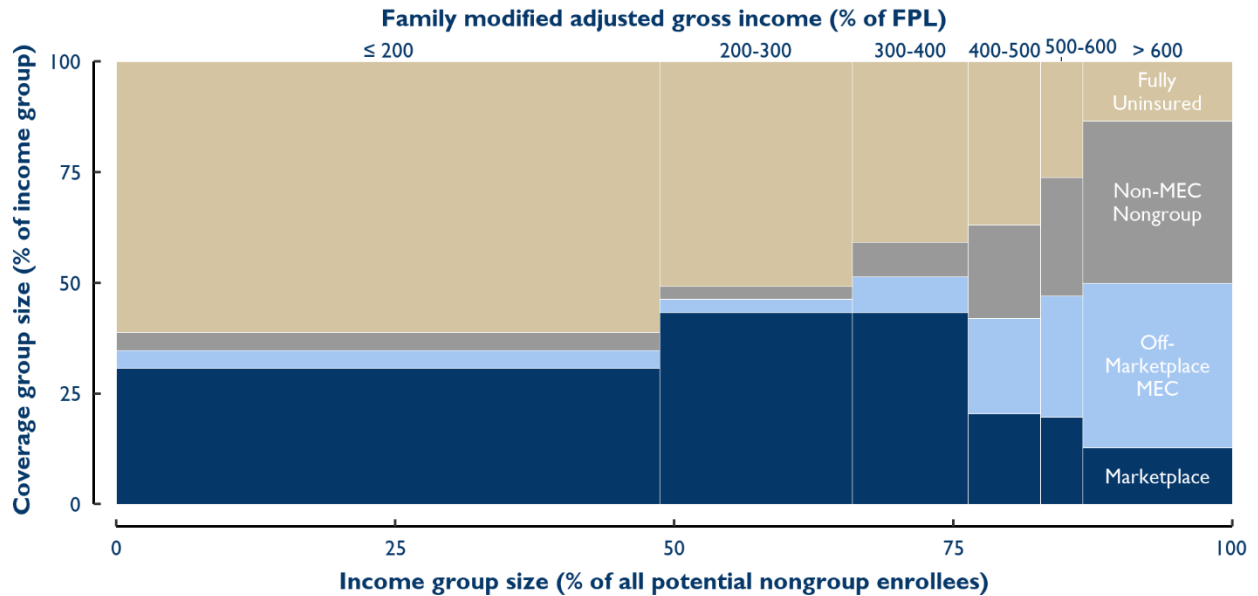
- *Share of uninsured in the coverage gap:* In this case, the events  $E_j$  and  $N_j$  should be taken to correspond to eligibility for *Marketplace* coverage. Paralleling the reasoning above, the fact that take-up of subsidized Marketplace coverage is incomplete implies that  $\mathbb{P}(T|N_0, E_p, Y_0 \in \mathcal{A})$  is meaningfully greater than zero. Since  $\mathbb{P}(T|E_0, N_p, Y_0 \in \mathcal{A})$  is also positive (albeit presumably larger), this implies that the spread between  $\mathbb{P}(T|E_0, N_p, Y_0 \in \mathcal{A})$  and  $\mathbb{P}(T|N_0, E_p, Y_0 \in \mathcal{A})$  may be meaningfully smaller than in the nongroup enrollment case, and the bias smaller as well.

The conclusion that bias in estimating the uninsurance-related shares may be modest is consistent with the evidence from the SIPP discussed in the main text that indicates that using full-year income versus monthly or expected income to impute program eligibility makes little difference in practice.

## Appendix E: Estimates for Broader Populations of Enrollees

This appendix reports estimates of nongroup enrollment and uninsurance for populations broader than the population of potential subsidy recipients examined in this paper's main analyses. Figure E.1 reports estimates for all non-elderly people who are ineligible for public or employer coverage (who I term potential nongroup enrollees), while Figure E.2 reports estimates for the full non-elderly population.

**Figure E.1: Potential Nongroup Enrollees by Income and Coverage, 2019**

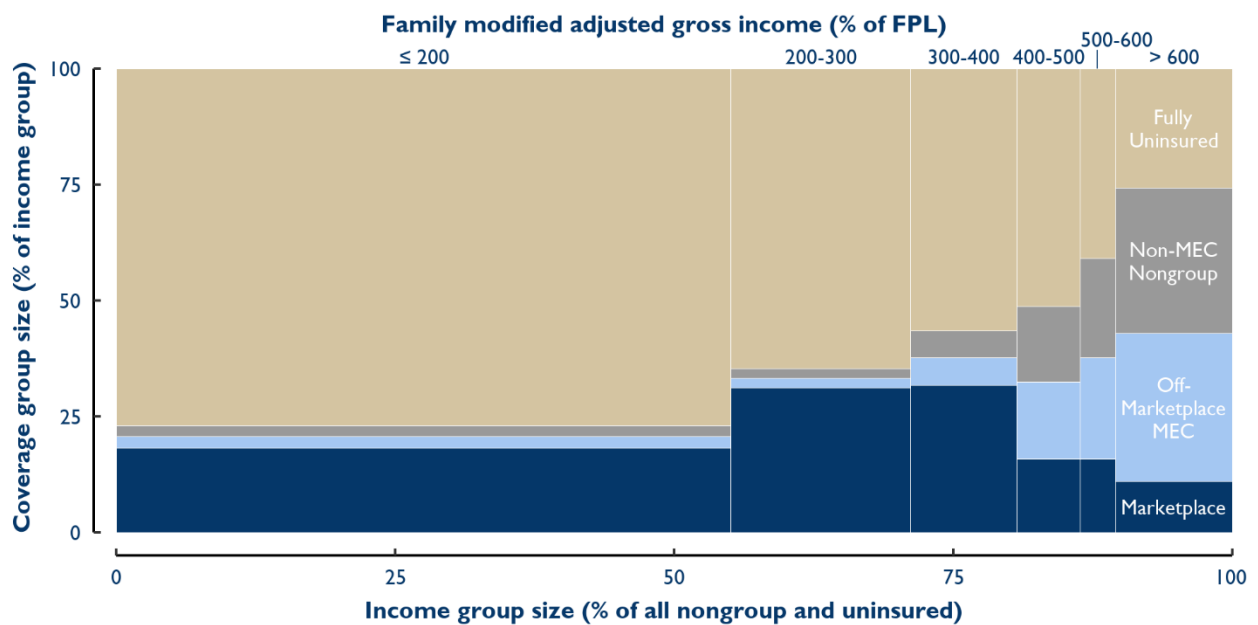


Note: Potential nongroup enrollees are people ineligible for public and employer coverage. Estimates include people under age 65 only. Enrollment measured in life-years. MEC = minimum essential coverage.

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**Figure E.2: Nongroup Enrollment and Uninsurance by Income, 2019**



Note: Estimates include people under age 65 only. Enrollment measured in life-years. MEC = minimum essential coverage.

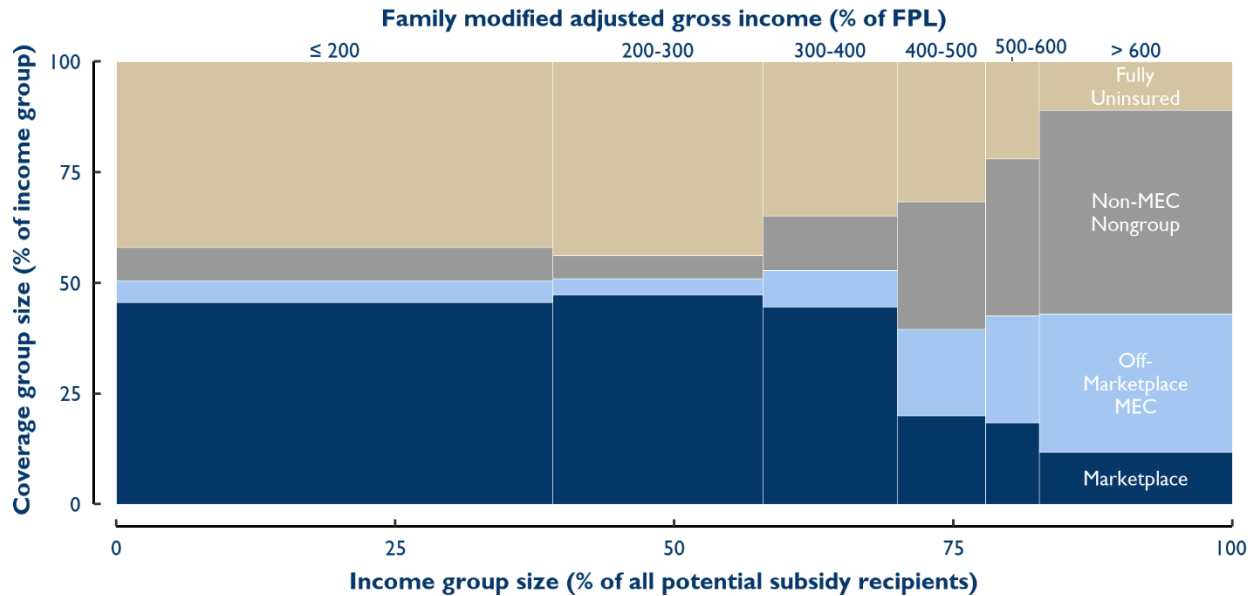
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## Appendix F: Sensitivity Analyses

This appendix presents the results of sensitivity analyses in which I assume that aggregate enrollment in non-MEC nongroup policies in 2019 is 50% higher or 50% lower than in my base estimates or, alternatively, that half of non-MEC nongroup enrollment is distributed proportionally to total MEC enrollment rather than all of that enrollment being distributed proportionally to off-Marketplace MEC enrollment.

**Figure F.1: Non-MEC Enrollment 50% Higher**

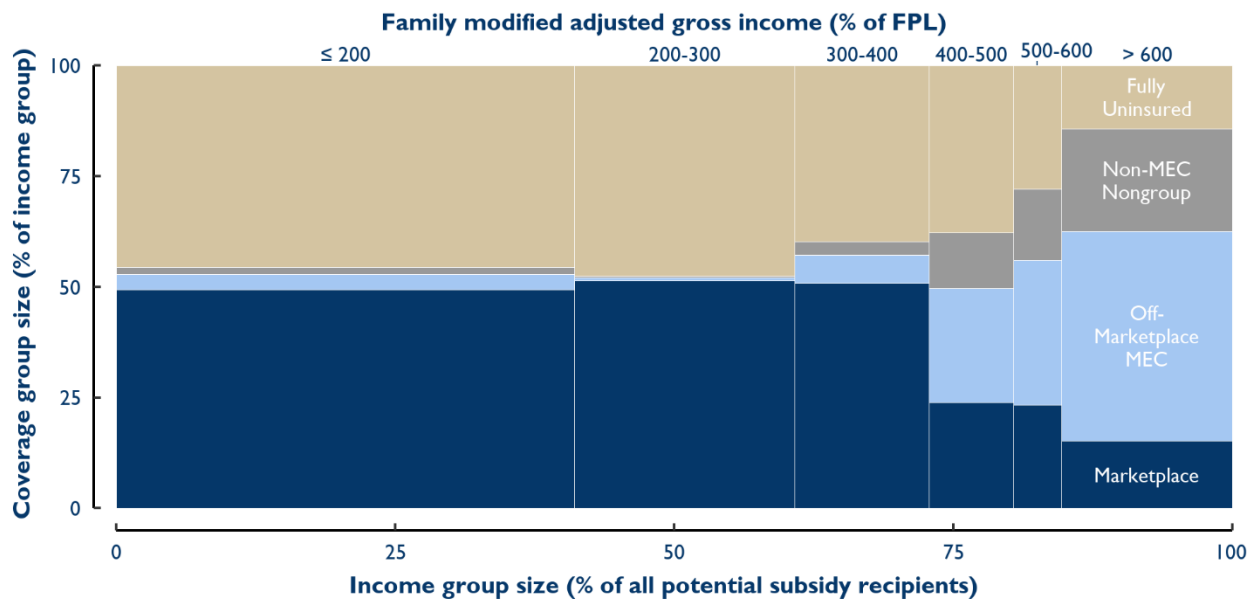


Note: Potential subsidy recipients are people who are legally present, ineligible for public and employer coverage, and not in the Medicaid "coverage gap." Estimates include people under age 65 only. Enrollment measured in life-years. MEC = minimum essential coverage.

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**Figure F.2: Non-MEC Enrollment 50% Lower**

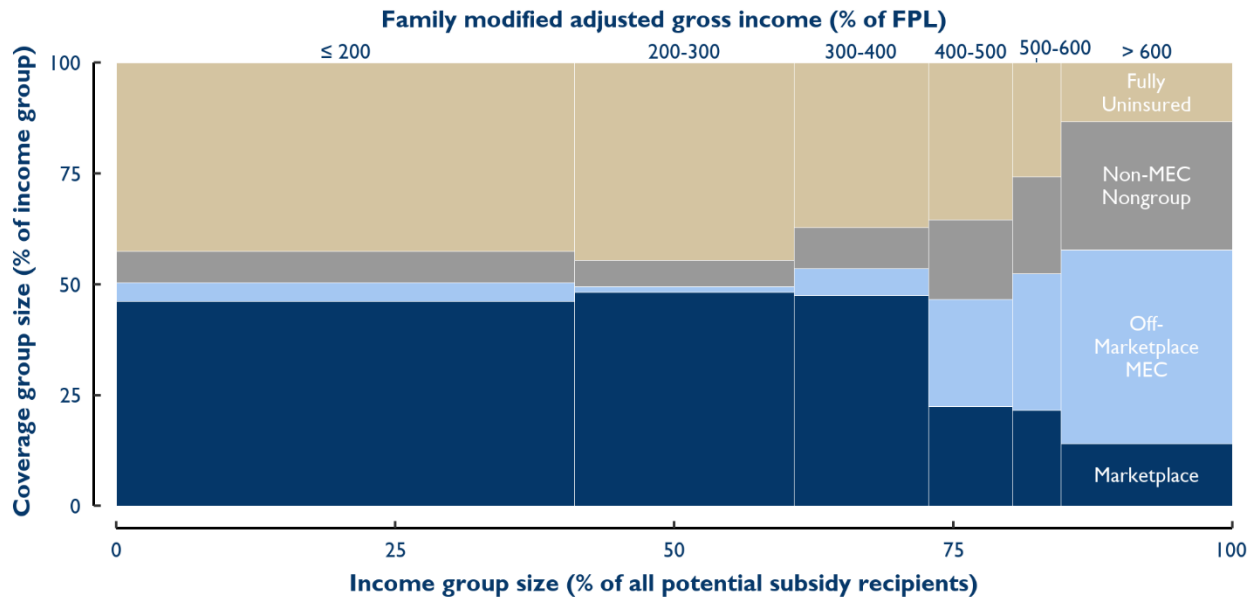


Note: Potential subsidy recipients are people who are legally present, ineligible for public and employer coverage, and not in the Medicaid "coverage gap." Estimates include people under age 65 only. Enrollment measured in life-years. MEC = minimum essential coverage.

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**Figure F.3: Distribute Half of Non-MEC Enrollment Using MEC Enrollment**



Note: Potential subsidy recipients are people who are legally present, ineligible for public and employer coverage, and not in the Medicaid "coverage gap." Estimates include people under age 65 only. Enrollment measured in life-years. MEC = minimum essential coverage.

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