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HEALTH INSURANCE AND MORTALITY: EXPERIMENTAL EVIDENCE FROM TAXPAYER OUTREACH*

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We evaluate a randomized outreach study in which the IRS sent informational letters to 3.9 million households that paid a tax penalty for lacking health insurance coverage under the Affordable Care Act. Drawing on administrative data, we study the effect of this intervention on taxpayers' subsequent health insurance enrollment and mortality. We find the intervention led to increased coverage during the subsequent two years and reduced mortality among middle-aged adults over the same time period. The results provide experimental evidence that health insurance coverage can reduce mortality in the United States. *JEL* Codes: I13, I18, H24.

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I. Introduction

The relationship between health insurance and mortality is a central question in the field of health economics and lies at the core of contemporary policy debates. Despite numerous studies, the issue remains hotly debated, at least in part because the causal effect of health insurance is difficult to credibly estimate.[1] Theoretically, the magnitude and even existence of an effect is ambiguous—for example, uninsured individuals with acute life-threatening conditions may still seek and obtain emergency care, which most hospitals in the United States are legally required to provide (42 U.S.C. §1395dd). Empirically, a growing number of well-designed quasi-experimental studies suggest that health insurance does substantially reduce adult mortality in certain contexts (Card, Dobkin, and Maestas 2009; Sommers, Baicker, and Epstein 2012; Sommers, Long, and Baicker 2014; Swaminathan et al. 2018; Khatana et al. 2019; Miller et al. 2019; Borgschulte and Vogler 2020), but the results that emerge from these studies rely on unverifiable and sometimes controversial assumptions (Levy and Meltzer 2008; Woolhandler and Himmelstein 2017; Black et al. 2019).[2] Prior randomized studies on this question are rare and inconclusive, both in the United States (Finkelstein et al. 2012, "the Oregon study"; Weathers and Stegman 2012) and worldwide.[3]

To shed light on this question, we evaluate a randomized outreach study conducted by the Internal Revenue Service (IRS). Near the beginning of 2017, the IRS sent informational letters to taxpayers who had previously paid an income tax penalty for

1. For examples of prominent studies that reach different conclusions on this question, compare Institute of Medicine (2002) with Kronick (2009). The issue is also disputed among policy makers, some of whom have expressed skepticism about whether health insurance reduces mortality (Phillips 2017).

2. An exception to the results in this line of research is Finkelstein and McKnight (2008), which finds no evidence that the introduction of Medicare reduced mortality for elderly adults. A separate body of quasi-experimental research finds health benefits from child health coverage, both contemporaneously (e.g., Currie and Gruber 1996; Goodman-Bacon 2018) and longer term (e.g., Wherry and Meyer 2016; Brown, Kowalski, and Lurie 2020).

3. Newhouse and the Insurance Experiment Group (1993) report evidence on mortality from an experiment that varied the amount of health insurance cost sharing rather than the presence or absence of coverage itself. Escobar, Griffin, and Shaw (2011) and Giedion, Alfonso, and Diaz (2013) survey international evidence on the link between health insurance and mortality and do not report the existence of any randomized studies.

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lacking health insurance coverage under the so-called individual mandate provision of the Affordable Care Act (ACA).[4] Of the 4.5 million households who met the criteria for inclusion in this pilot program, 3.9 million were randomly selected to be sent a letter. Because the intervention led to increased health insurance enrollment among this group, it provides an opportunity to explore the causal relationship between health insurance and mortality.

Beginning with the effect of the intervention on coverage, we find that among individuals who were uninsured for some portion of the prior year, those in the treatment group were 1.1 percentage points more likely to enroll in coverage during the two years following the intervention than those in the control group, a 1.9% relative increase. On average, the intervention induced an average of 0.23 months of additional coverage during this time period, or one additional year of coverage for every 52 treated individuals. The increase in coverage was primarily driven by increased enrollment in the individual marketplace, and, to a lesser extent, new take-up of Medicaid. We document larger effects among individuals who were sent letters with more time remaining before the enrollment deadline and among those whose letters included personalized penalty estimates. Although there is some attenuation, the coverage rate remained higher in the treatment group than in the control group during the two years following the intervention.

In the second part of the article, we study whether the additional coverage induced by the intervention reduced mortality among those who enrolled. We find evidence that it did. In the two years following the intervention, the mortality rate among previously uninsured 45–64-year-olds was lower in the treatment group than in the control group by approximately 0.06 percentage points, or one fewer death for every 1,587 treated individuals. We find no evidence that the intervention reduced mortality among children or younger adults during this period. Exploiting treatment group assignment as an instrument for coverage, we estimate that the average per-month effect of the coverage induced by the intervention on two-year mortality was approximately −0.18 percentage points. We caution, however, that the magnitude of the mortality effect is imprecisely estimated; our

4. From 2014 to 2018, federal law required most individuals to enroll in health insurance coverage or pay a tax penalty. As with other taxes, the IRS played a central role in administering this provision, including explaining its operation to taxpayers.

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confidence interval is consistent with both moderate and large effects of coverage. At the same time, our estimated confidence interval is sufficiently precise to rule out per-month effects smaller in magnitude than −0.04 percentage points, including the estimate from the OLS regression of mortality on coverage across individuals. We view the effects at the lower magnitude end of our confidence interval as most plausible, given the treatment effect magnitudes reported in prior research (see Black et al. 2019 for a review). We also present suggestive evidence that the effect of annual coverage on mortality is less than 12 times our estimated per-month effect due to concavity in the relationship between coverage and mortality. With these caveats, our results provide experimental evidence that the coverage induced by the IRS intervention reduced mortality among middle-aged adults.

Our study makes several contributions. First, it provides new evidence on the link between health insurance and mortality. Because our research design exploits random assignment, it is not subject to most of the identification concerns that have been leveled against nonexperimental research in this area. In addition, our study complements the prior experimental evidence in important ways. Because mortality is a rare event, statistical power poses a significant concern for both experimental and nonexperimental analyses of the topic. The size of our sample permits us to restrict our main mortality analysis to middle-aged adults, a population for which the protective effects of coverage on mortality are more likely to be detectable. In addition, the group for which we document an effect of coverage—that is, those who already had access to coverage but for whom misperceptions or other frictions prevented take-up—is particularly policy relevant, since such individuals' coverage decisions can be shaped through outreach efforts of the type commonly employed by governments and nonprofits. Indeed, our experimental sample represents a substantial share of the uninsured individuals in the United States who are the typical targets of such efforts.

Our second contribution is to shed light on the operation of tax incentives for promoting health insurance. A number of studies investigate the effect of the individual mandate and related policies on health insurance enrollment (e.g., Hackmann, Kolstad, and Kowalski 2015; Frean, Gruber, and Sommers 2017; Lurie, Sacks, and Heim forthcoming). Our findings provide evidence that informational or behavioral frictions like low salience or complexity can limit the effectiveness of such policies (if not, our intervention

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would not have had an effect). In this sense, our results complement recent studies that document mistakes in other domains of health-related decisions (summarized in Chandra, Handel, and Schwartzstein 2019) and with respect to tax incentives more generally (Chetty, Looney, and Kroft 2009). In a different vein, some have suggested that the dollar value of the federal mandate penalty may have been too small to influence behavior (Auerbach et al. 2010; Frean, Gruber, and Sommers 2016); our coverage results provide evidence against that view, at least with respect to the 2017 federal penalty. Although the individual mandate is now effectively repealed at the federal level, exploring these issues is important for assessing proposals to impose (or reimpose) an individual mandate at the state or federal level (Levitis 2018) and for informing related outreach efforts (Dorn 2019).

Methodologically, our analysis illustrates how large-scale field experiments can provide information not only about the intervention being studied but also about downstream effects of the behavior the intervention induces. In particular, health insurance access is expensive to provide, but without randomization credible estimates of the effect of health insurance on health are difficult to obtain. By exploiting a randomized outreach program that encouraged individuals to take up coverage that was already available to them, our approach represents a cost-effective and ethical method for studying the effect of coverage on subsequent health outcomes.

Finally, we highlight a new data source for studying health insurance in the United States: information returns about individual coverage reported to the IRS. Prior research on this topic has mostly relied on self-reported survey data for a small fraction of the population or administrative coverage data obtained from a single insurer. Our data set offers several important advantages. First, it covers the near universe of individuals living in the United States, which limits concerns about selection and endogenous sample retention. Second, because the data contain individual-level coverage information from multiple insurers, we can observe flows between insurance providers or types of coverage. Third, the monthly frequency of the data allows us to more precisely quantify the first-stage effects of interventions like the one we study and the dynamics of treatment effects over time. Because most prior studies have lacked such precise measures of coverage, they have typically focused on a binary indicator for being enrolled in any coverage at a single point in time or during the year, which can bias instrumental variable estimation when an instrument affects coverage on other margins.

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The remainder of the article proceeds as follows. Section II provides institutional background on health insurance coverage in the United States and the federal individual mandate. Section III describes our research design and provides information about our data, the outreach study, and summary statistics. Section IV analyzes the effect of the intervention on coverage. Section V contains the mortality analysis. Section VI concludes. An Online Appendix contains supplementary results.

II. Institutional Background

II.A. Health Insurance Coverage in the United States

Most people in the United States with health insurance have coverage from their employer (employer-sponsored insurance, ESI). The second-largest source of health insurance is through a government program such as Medicaid, Medicare, or the Veterans Administration. Individuals who do not receive health insurance from one of these sources may enroll in an Exchange plan, purchased through their state's health insurance marketplace, or in an Off-Exchange plan.

To understand how the intervention affected coverage decisions, it is helpful to understand the constraints on the timing of health insurance enrollment. Unless special circumstances apply, individuals may enroll in Exchange coverage only during the open enrollment period for the applicable year.[5] For 2017, the open enrollment window was from November 1, 2016, to January 31, 2017. In addition, individuals are required to apply for Exchange coverage by the 15th day of the month prior to the month in which coverage is to begin. Thus, to obtain coverage for January 2017, individuals must have applied by December 15, 2016; to obtain coverage for February 2017, individuals must have applied by January 15, 2017. Most employer-sponsored plans also have an annual open enrollment period to enroll in coverage for the year, often ending a month or two prior to the end of the calendar year. In contrast, individuals can obtain Medicaid coverage at any time during the year (e.g., during a hospitalization), and Medicaid coverage can apply retroactively for up to three months prior to the month of application.

5. Examples of events that permit an individual to enroll in coverage outside of the open enrollment window include losing health coverage (e.g., by losing one's job), getting married or divorced, or the birth of a new child.

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Most individuals apply for Exchange coverage through an online portal. States may rely on the federal marketplace or operate their own state-based marketplace. Individuals who visit the federal online portal, healthcare.gov, are routed to the applicable marketplace based on their location. Exchange enrollees may be eligible for a tax credit to help pay for premiums. To determine the after-subsidy cost of Exchange coverage, individuals who visit the Exchange input information about their family size and income. If the household qualifies for Medicaid, they can enroll in that coverage through the website as well.

In recent years, the share of Americans under the age of 65 who lack health insurance coverage for the entire year is approximately 9%–13%, depending on the source of the estimate.[6] The share of Americans who lack coverage for one month or more during the year is much higher, approximately 21% to 26%. Among those with full-year coverage in 2016, 65% received at least one month of coverage from an employer, 35% received at least one month of government-provided coverage, and 10% were enrolled in at least one month of nongroup coverage (i.e., from an Exchange or off-Exchange plan).

II.B. The Individual Mandate

The Shared Responsibility Payment provision of the ACA, or the individual mandate (as it is commonly referred to), required most individuals to obtain health insurance coverage or pay a penalty. Individuals who lacked qualifying health insurance coverage for themselves or a dependent for one or more months during the year were required to report and pay the penalty on their annual income tax return unless an exemption applied. Exemptions were available under a range of circumstances, including unaffordability of the available coverage to the individual, income below the income tax filing threshold, general hardship, individuals living abroad, religious objections, and gaps in coverage of less than three months.[7]

6. The statistics cited in this paragraph are reported in Lurie and Pearce (2019).

7. Under the last of these exemptions, an individual could avoid the penalty if she enrolled in coverage for only 10 months during a year, as long as the 2 months without coverage were consecutive and not part of a longer gap spanning multiple years. For additional details relating to the exemptions, see Lurie and McCubbin (2016).

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The amount of the annual penalty owed by a taxpayer depended on the taxpayer's income, household size, and number of months without coverage. The specific parameters varied by year, but in 2017 (the focus of our intervention), the penalty for taxpayer i for month m, P_{im}, was given by:

$$P_{im} = \frac{1}{12} \max \{\min \{695\, A_i + 347.5\, B_i,\ 2085\},\ 0.025\ (I_i - F_i)\},$$

where A_i is the number of adults on i's return who lacked coverage, B_i is the number of children on i's return who lacked coverage, F_i is the applicable filing threshold for i, and I_i is a measure of i's income during the year.[8] In turn, i's penalty for the year, P_i, is the sum of i's monthly penalties, limited by the yearly premium for the national average lowest cost bronze plan that was available to i's household ($LCBP_i$):

$$P_i = \min \left\{ \sum_{m=1}^{12} P_{im},\ LCBP_i \right\}.$$

In December 2017, President Trump signed the Tax Cuts and Jobs Act, which, among other changes to the tax code, effectively eliminated the individual mandate for 2019 and subsequent years. In response, a number of states have enacted or are considering enacting legislation at the state level that would resemble the operation of the federal individual mandate. To date, California, New Jersey, Rhode Island, Vermont, and Washington, DC, have already enacted such legislation. Massachusetts adopted an individual mandate as part of its 2006 health reform, and it remains in effect today (see Levitis 2018).

Even while the individual mandate was in operation, several aspects of its design may have limited its effect on individuals' coverage decisions. First, some individuals may have neglected to consider the penalty when deciding whether to enroll in coverage, either because the penalty was not salient or because they lacked knowledge of its existence. Because the penalty appeared as a single line on one's tax return, even those who paid it may not have known of its existence, especially if they used software or a third party to prepare their return. Second, the penalty formula was quite complex—as described above, the penalty was

8. Specifically, I_i refers to modified adjusted gross income, which for purposes of the individual mandate is equal to adjusted gross income plus untaxed foreign income and tax-exempt interest.

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the minimum of a maximum of a minimum—which may have weakened its effect on behavior. Third, the timing of the penalty may have rendered it less effective; even if the penalty was salient to individuals when they paid their taxes (typically February through April), they may have forgotten about it by the close of the calendar year when the next open enrollment window began. These concerns motivated the design of the outreach intervention.

III. Data and Research Design

This section describes our data, the experimental sample, and the outreach intervention, and provides summary statistics and balance checks.

III.A. Data Sources

For our analysis, we rely on administrative records from population files housed at the IRS. These data include annual information on the universe of individuals who file or are listed on individual tax returns, as well as annual and subannual information on the universe of individuals listed on information returns.

Our health insurance coverage data are derived from information returns (Form 1095 A/B/C), which provide monthly coverage information at the individual level. These forms are provided annually to the IRS by private and public insurers, self-insured employers, and health insurance marketplaces, who are required to provide a form for each individual they cover. In addition to allowing us to identify whether an individual was covered during a particular month, the forms provide information about the type of coverage in which the individual was enrolled (e.g., whether the coverage was Medicaid). Only certain health insurance, referred to as minimum essential coverage, satisfied the individual mandate. This is the only type of coverage reported on the Form 1095s. Examples of health insurance plans that do not constitute minimum essential coverage include plans limited to vision or dental care, workers' compensation, or coverage limited to a specific disease or condition. For additional details about the coverage data on which we rely, refer to Lurie and Pearce (2019).

Our measure of health insurance coverage is available monthly from January 2015 through December 2018. We assume that individuals for whom no Form 1095 was received for a year were uninsured during each month of that year.

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Our data on mortality comes from the Social Security Death File, which records the universe of U.S. deaths along with the date at which the death occurred.[9] Unfortunately, the data do not contain information about the cause of death.

III.B. Sample Construction

To construct our sample, we first identified tax returns filed for 2015 that reported owing a positive penalty under the individual mandate. Approximately 6.1 million 2015 tax returns fell into this category.[10] For context, in the same year, the total number of filed tax returns was approximately 140 million, of which 22.8 million did not indicate full-year coverage for each person listed on the return.[11]

We excluded returns that satisfied one or more of the following conditions: the taxpayer was claimed as a dependent on a different tax return; the filing address was not from one of the 50 U.S. states or D.C.; the taxpayer filed multiple (nonamended) 2015 returns; the filing address listed a second address line (typically "C/O"); the return listed an Individual Taxpayer Identification Number instead of a Social Security Number for a taxpayer or dependent; the taxpayer was over age 64 or under age 18 at the end of 2017; the taxpayer's account was subject to certain audit or examination codes; a household member listed on the return was observed to have enrolled in Exchange coverage either in 2015 or in 2016 prior to our sample being finalized; or the taxpayer was recorded as dead prior to the date of sample construction.[12]

9. The number of deaths reported in these data closely tracks those reported by the National Center for Health Statistics (Online Appendix Figure A.I). Chetty et al. (2016) report correlations between the two data sources exceeding 0.98 across ages and years.

10. This quantity refers to the set of 2015 tax returns that had been filed and posted on the IRS system as of October 2016, when the sample was constructed. Some additional 2015 tax returns were filed after that date and some previously filed 2015 returns were subsequently amended to include a penalty.

11. Among those tax returns that did not indicate full-year coverage for each household member and that did not report owing a penalty, approximately 11.3 million claimed an exemption for one or more months of the year. Among the remaining approximately 5.4 million returns, some reported a penalty but were filed after our sample was constructed, whereas others failed to either claim an exemption or report a penalty.

12. At the time the sample was constructed, the version of the Social Security Death File used by the IRS reflected deaths recorded through mid-July 2016; hence, most individuals who died before that date were excluded from the sample. However, some individuals who died before that date were included in the sample

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Online Appendix Table A.I describes the rationale for these exclusions and the number of tax returns affected. As discussed further below, every individual listed on a return is included in our analysis: the primary taxpayer, the spouse if married and filing jointly, as well as up to four dependents (who may be younger than 18 or older than 64). The final sample consists of 4.5 million returns, corresponding to 8.9 million individuals.

III.C. Outreach Intervention

The outreach intervention was a joint project designed primarily by the Treasury Department's Office of Tax Analysis, funded by the Department of Health and Human Services (HHS), and implemented by the IRS. The intervention took the form of an informational letter sent to taxpayers from the IRS. The letter informed recipients that they had paid a penalty in 2015; provided information about the penalty and plan costs for 2017; and provided instructions for recipients to investigate the availability of Exchange and Medicaid coverage through healthcare.gov. A sample letter is provided in Online Appendix Figure A.II.

Taxpayers in the sample were randomly assigned to receive a letter (86%) or to a control group (14%). One letter, addressed to the taxpayer(s), was sent per tax return. Randomization was stratified by the age and gender of the primary filer, marital status, number of dependents, income, the presence of self-employment income, 2014 penalty status, and whether the taxpayer's state expanded Medicaid and/or participated in the federal marketplace during 2017. We note that taxpayers assigned to the control group may have been exposed to other outreach efforts conducted by HHS, state agencies, or nonprofits, such as television or radio advertisements, direct mailings, or in-person canvassing.[13]

The decision to randomly assign a subset of the sample to a control group was the product of two main factors. First, a randomized design would facilitate evaluating the effectiveness of the outreach to inform decisions about undertaking similar efforts in subsequent years. Second, given the number of taxpayers

because their deaths were recorded in the Social Security Death File with a lag. In addition, the sample includes dependents listed on the 2015 return who died before the date of sample construction.

13. The IRS conducted other outreach during the same time period focused on other populations of taxpayers, such as those who failed to claim tax credits for which they appeared eligible or who did not pay a penalty but did claim an exemption under the individual mandate.

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that paid a penalty in 2015, the control group ensured that the intervention remained within its allotted budget. The shares of the sample assigned to the treatment and control groups were the product of discussions between the agencies involved in the intervention that balanced these factors against the program objective of broad outreach.

The taxpayers selected to receive a letter were randomly assigned across several treatment arms. The baseline treatment contained a personalized estimate of the taxpayer's potential 2017 penalty (based on 2015 income and household composition) and was mailed in mid-January 2017, approximately two weeks before the close of the open enrollment period. A nonpersonalized treatment variant was identical to the baseline treatment except that the letter provided generic information about the 2017 penalty formula rather than a personalized estimate. An exemption information treatment variant was identical to the baseline treatment, but included additional information about penalty exemptions for which the taxpayer might be eligible. An early mailing treatment variant was identical in content to the baseline treatment but was mailed in late November 2016, near the start of the open enrollment period. Based on operational considerations, approximately 21% of the treatment group was assigned to the early mailing variant. The remainder of the treatment group was randomly divided among the baseline treatment and the other two variants in equal proportions. Finally, the baseline treatment and each of the three variants were randomly divided into two equal-sized groups, one of which had a Spanish-language translation printed on the reverse side of the letter and one that did not. Online Appendix Table A.II summarizes the allocation of the sample across treatment arms.

III.D. Summary Statistics and Balance Checks

Table I contains individual-level summary statistics for the experimental sample and information about covariate balance. As benchmarks, columns (1) and (2) provide summary statistics for a random 1% sample of the overall population of tax returns (column (1)) as well as for the full population of returns that did not indicate full-year coverage for 2015 (column (2)).[14] Relative to these baseline populations, individuals in the experimental

14. The latter category contains taxpayers who claimed an exemption and taxpayers who owed but did not report a penalty.

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TABLE I

SUMMARY STATISTICS AND BALANCE CHECKS

			Experimental Sample			
	All taxpayers (1)	No full-year coverage (2)	All (3)	Treatment (4)	Control (5)	Difference p-value (6)
Individual characteristics						
Female	0.511	0.478	0.450	0.450	0.451	.679
Age (years)	38.6	34.5	31.1	31.1	31.1	.410
0–18	0.239	0.265	0.271	0.271	0.271	.384
19–26	0.111	0.128	0.136	0.136	0.136	.771
27–44	0.242	0.290	0.349	0.349	0.349	.684
45–64	0.261	0.230	0.230	0.230	0.230	.977
65 or older	0.147	0.087	0.014	0.014	0.014	.506
Household characteristics						
Married	0.554	0.450	0.414	0.414	0.414	.863
Household income	78,534	30,159	42,709	42,697	42,782	.346
Income < 138% FPL	0.366	0.657	0.267	0.267	0.266	.136
Household size	2.81	2.86	2.74	2.74	2.74	.741
Self-prepared returns	0.414	0.437	0.341	0.341	0.341	.827
Local characteristics						
High school degree or higher	0.866	0.835	0.835	0.835	0.835	.553
BA degree or higher	0.299	0.264	0.249	0.249	0.249	.335
Expansion state	0.618	0.523	0.560	0.560	0.560	.822
State-based marketplace	0.344	0.288	0.222	0.222	0.222	.637
Penalty						
Claimed 2014 exemption	0.088	0.307	0.175	0.176	0.174	.009
Paid 2014 penalty	0.055	0.196	0.425	0.425	0.425	.736
2014 penalty if penalized	247	246	257	257	258	.178
Claimed 2015 exemption	0.076	0.443	0.063	0.063	0.063	.143
Paid 2015 penalty	0.048	0.300	1.000	1.000	1.000	.
2015 penalty if penalized	545	546	528	528	529	.265
Projected 2017 annualized penalty	2,109	1,599	1,526	1,526	1,526	.936
2015 coverage (Jan–Dec)						
Any coverage	0.863	0.594	0.586	0.586	0.586	.314
Covered months	9.80	5.88	5.30	5.30	5.30	.352
Full-year coverage	0.745	0.356	0.283	0.283	0.283	.665
2016 coverage (Jan–Nov)						
Any coverage	0.889	0.649	0.638	0.638	0.637	.453
Covered months	9.38	6.35	6.02	6.02	6.01	.210
Full-year coverage	0.794	0.479	0.428	0.428	0.427	.095
Joint test (p-value)						.516

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TABLE I
CONTINUED

			Experimental Sample			
	All taxpayers (1)	No full-year coverage (2)	All (3)	Treatment (4)	Control (5)	Difference p-value (6)
Observations						
Individuals	2,893,655	45,472,192	8,893,653	7,647,822	1,245,831	
Households	1,398,008	22,778,960	4,526,717	3,892,847	633,870	

Notes. The table presents summary statistics for a 1% random sample of 2015 tax returns (column (1)), the set of 2015 tax returns that did not report full-year coverage (column (2)), and the set of 2015 tax returns included in the experimental sample (columns (3)–(5)). Column (6) reports the p-value for the test of equality between the treatment and control groups, with standard errors clustered by household. The joint test p-value corresponds to the null of equality between the treatment and control groups for all reported characteristics. All statistics are calculated at the individual level. Age refers to the individual's age at the end of 2015. Local characteristics are imputed based on the ZIP code corresponding to the individual's 2015 tax return. Household refers to the taxpayers and dependents listed on the individual's 2015 tax return; household characteristics are derived from information reported on that return. Income refers to modified adjusted gross income. FPL refers to the applicable federal poverty line, calculated from the household size and state corresponding to the 2015 tax return. Self-prepared return indicates whether the 2015 tax return was prepared by a third-party preparer. The projected 2017 annualized penalty refers to the estimated individual mandate penalty a household would owe if all household members lacked coverage during 2017, based on household size, location, and income reported in 2015. Local characteristics are imputed based on the ZIP code corresponding to the individual's 2015 tax return. Expansion state refers to whether the reported state of residence had expanded Medicaid as of January 1, 2017. Coverage is measured from Form 1095 A/B/C. For 2015 coverage: any coverage indicates one or more month of coverage during the year; covered months refers to the number of months of the year in which the individual was enrolled in coverage; and full-year coverage indicates the individual was enrolled in 12 months of coverage during the year. For 2016 coverage, these measures are defined analogously except they are defined based on coverage during the first 11 months of the year.

sample (column (3)) are younger, more likely to be male, and less likely to be married. Notably, although the experimental sample is much lower income than the overall population, it is higher income on average than the population without full-year coverage, many of whom qualified for an income-based exemption from the penalty.

Although inclusion in the experimental sample implies that at least one individual in the household lacked coverage in at least one month of 2015, that individual may have had coverage for other months of the year, and others in the household may have been enrolled in coverage during every month of the year. Table I shows that over half (59%) of those in the experimental sample had coverage for at least one month in 2015 and a substantial minority (28%) had coverage during every month of that year. Along both of these measures, the fraction of our sample with coverage rose from 2015 to 2016.[15] Finally, note

15. Table I reports coverage statistics for the first 11 months of 2016 because the intervention may have shaped coverage decisions for December 2016.

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that in both 2015 and 2016, most individuals had either full-year coverage or zero months of coverage.[16]

Table I, columns (4)–(6) investigate covariate balance between the treatment and control groups. Consistent with the randomized design and large sample sizes, the treatment and control groups are quite similar in most respects. A test of joint significance across all variables fails to reject the null of equality between the treatment and control groups ($p \approx .52$).

IV. Coverage Effect

This section investigates the effect of the outreach intervention on individuals' subsequent coverage decisions.

Table II, Panel A presents our primary results relating to health insurance coverage. For ease of interpretation, we initially pool individuals assigned to any of the treatment arms into a single treatment category. Under our randomized design, the coverage effect is identified by comparing the means of the coverage outcomes across the treatment and control groups; however, controlling for a range of prerandomization demographic and geographic variables yields similar results (Online Appendix Table A.IV), as does controlling for randomization strata indicators (Online Appendix Table A.V). Because individuals listed on the same tax return were assigned to the same treatment group, standard errors are clustered at the household level.

Table II, columns (1) and (2) focus on the full experimental sample. Overall, the intervention caused individuals to enroll in an average of 0.15 additional months of coverage per person during the two years following the intervention, an approximately 1% increase relative to the control group mean of 14 months (column (1)). The effect of the intervention is precisely estimated; the 95% confidence interval ranges from approximately 0.13 to 0.18 coverage-months. Along similar lines, column (2) shows that the intervention increased the likelihood of obtaining at least 1 month of coverage during 2017 or 2018 by 0.69 percentage points, an approximately 1% increase relative to the control group mean of 75%.

16. Online Appendix Table A.III provides comparison statistics for nonfilers aged 19 to 64 who were listed on information returns. Such nonfilers tend to be older, lower income, and more likely to be male than the tax-filing population and the experimental sample. Despite these differences, nonfilers were insured at similar rates as the overall tax-filing population. In 2015 nonfilers accounted for approximately 16.3% of the population that lacked insurance during at least one month and 17.4% of the population that was uninsured for the full year.

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TABLE II
COVERAGE EFFECT BY PRIOR-YEAR INSURANCE

	Full sample		Prior-year insured		Prior-year uninsured	
	Months of coverage (1)	Any coverage (2)	Months of coverage (3)	Any coverage (4)	Months of coverage (5)	Any coverage (6)
Panel A: All ages						
Treated	0.152	0.685	0.018	0.029	0.232	1.107
	(0.013)	(0.052)	(0.011)	(0.025)	(0.016)	(0.077)
Control mean	14.410	75.431	20.970	98.072	9.512	58.525
Observations	8,893,653	8,893,653	3,809,488	3,809,488	5,084,165	5,084,165
Panel B: Middle-aged adults						
Treated	0.271	1.286	0.052	0.040	0.358	1.831
	(0.024)	(0.105)	(0.022)	(0.053)	(0.026)	(0.135)
Control mean	12.286	65.223	21.189	97.869	7.795	48.753
Observations	2,047,778	2,047,778	688,795	688,795	1,358,983	1,358,983

Notes. The table reports the effect of the intervention on health insurance coverage enrollment. In columns (1), (3), and (5), the outcome is months of coverage during 2017–2018. In columns (2), (4), and (6), the outcome indicates enrollment in one or more month of coverage during 2017–2018; units are percentage points (0–100). Columns (3) and (4) limit the analysis to individuals enrolled in coverage during each of the first 11 months of 2016. Columns (5) and (6) limit the analysis to individuals that were not enrolled in coverage during at least 1 of the first 11 months of 2016. Panel B limits the analysis to individuals between the ages of 45 and 64 at the end of 2017. Standard errors, reported in parentheses, are clustered by household.

As described above, approximately three-quarters of the sample would have enrolled in at least some coverage during the outcome period even absent the intervention. Although we cannot identify which individuals fall into this category, one proxy might be the number of months of coverage in which the individual was enrolled during the prior year (2016). Table II, columns (3) and (4) explore this possibility by restricting the analysis to the 43% of the sample that were fully insured during 2016. Because the intervention may have affected coverage decisions for December 2016, we define our measure for whether an individual was fully insured during 2016 by whether they were enrolled in coverage during each of the first 11 months of that year. Among individuals in this category, the vast majority (98%) obtained at least some coverage during the outcome period, even absent the intervention. Correspondingly, we estimate that the intervention increased coverage for individuals with full prior-year coverage by only 0.02 months (column (3)) and the likelihood of obtaining any coverage by 0.03 percentage points (column (4)).

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Because there was limited scope for the intervention to affect those with full prior-year coverage, Table II, columns (5) and (6), as well as most subsequent analyses, restrict our sample to exclude this group. Among the prior-year uninsured (i.e., those who lacked coverage in at least one of the first 11 months of 2016), the intervention increased coverage by 0.23 months on average (column (5)) and increased the likelihood of obtaining any coverage by 1.1 percentage points (column (6)). Under the assumption that the intervention did not cause any individuals to reduce coverage, these point estimates are consistent with as many as 23.2% of people enrolling in additional coverage because of the intervention or as few as 1.2% doing so (Online Appendix Table A.VI).[17] At a combined printing and postage cost of approximately $0.49 per letter, our estimated coverage effect implies an average outreach cost of $12.94 per additional year of coverage induced by the intervention among the prior-year uninsured during the outcome period.[18]

Table II, Panel B presents coverage effects for the middle-aged (45–64-year-old) sample that we will focus on in Section V when we investigate the effect of the intervention on mortality. Although the between-column patterns are similar across panels, we observe substantially larger coverage effects for each column in Panel B compared with the corresponding column in Panel A. For example, the effect of the intervention on coverage-months during the outcome period was 0.36 among the middle-aged previously uninsured, over 50% higher than our estimated effect for the overall previously uninsured population.[19] That older adults responded more to the intervention is striking given that younger

17. These bounds are derived by comparing the marginal distributions of coverage-month enrollment among the treatment and control groups, following Huang et al. (2017).

18. At $0.49 per letter and 1.96 individuals per household, the average cost per treated individual is approximately $0.25. The estimated coverage effect therefore implies an average cost of $1.08 per additional coverage-month, or $12.94 for 12 additional months of coverage. This cost estimate does not account for staff time associated with the implementation or initial development of the intervention or the budgetary cost of new enrollment into subsidized or free coverage. Including individuals with full-year 2016 coverage in this calculation (as in Table II, column (1)) implies an outreach cost of $19.75 per year of coverage induced.

19. Assuming the effect of the intervention on coverage was weakly monotonic for this group, the point estimates in Panel B, columns (5) and (6) are consistent with anywhere between 1.9% and 29.0% of previously uninsured middle-aged adults enrolling in additional coverage because of the intervention (Online Appendix Table A.VI).

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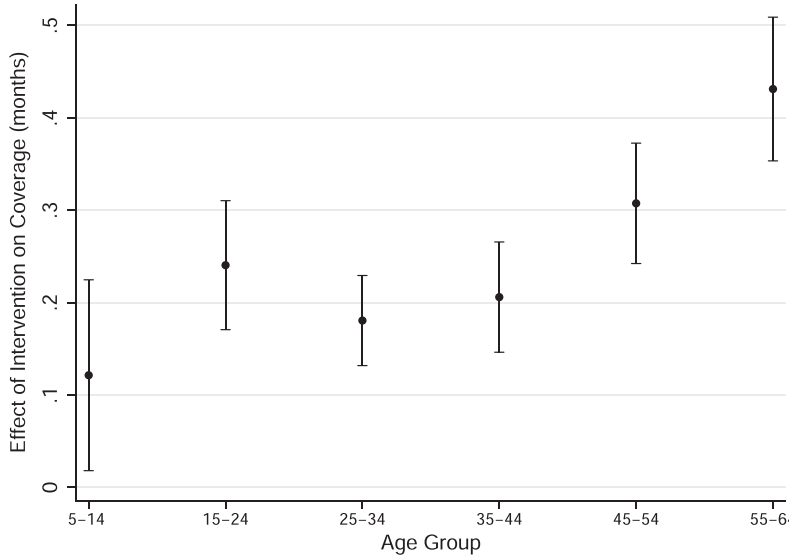


FIGURE I

Coverage Effect by Age

The figure displays the estimated effect of the intervention on coverage by age. The outcome is the number of months of coverage enrolled in during 2017–18. Each specification is limited to individuals whose ages fell into the specified range at the end of 2017. The figure excludes individuals with full coverage in January through November of 2016. Brackets denote the 95% confidence interval based on standard errors that are clustered by household.

adults were more likely to forgo coverage (as reflected in the respective control group means). This pattern could emerge if some of those who lacked coverage in the years prior to reaching Medicare eligibility were intentionally deferring consumption of health services (Card, Dobkin, and Maestas 2008; Freed 2017), but, absent the intervention, were overestimating the net financial cost of coverage. Figure I provides additional detail about the observed upward-sloping relationship between age and the effect of the intervention on coverage.

To explore the timing of the observed coverage effect, Figure II, Panel A separately plots monthly coverage rates for the treatment and control groups and Panel B plots the monthly difference in coverage rates.[20] Consistent with the randomized

20. We present corresponding figures for the middle-aged sample in Online Appendix Figure A.III.

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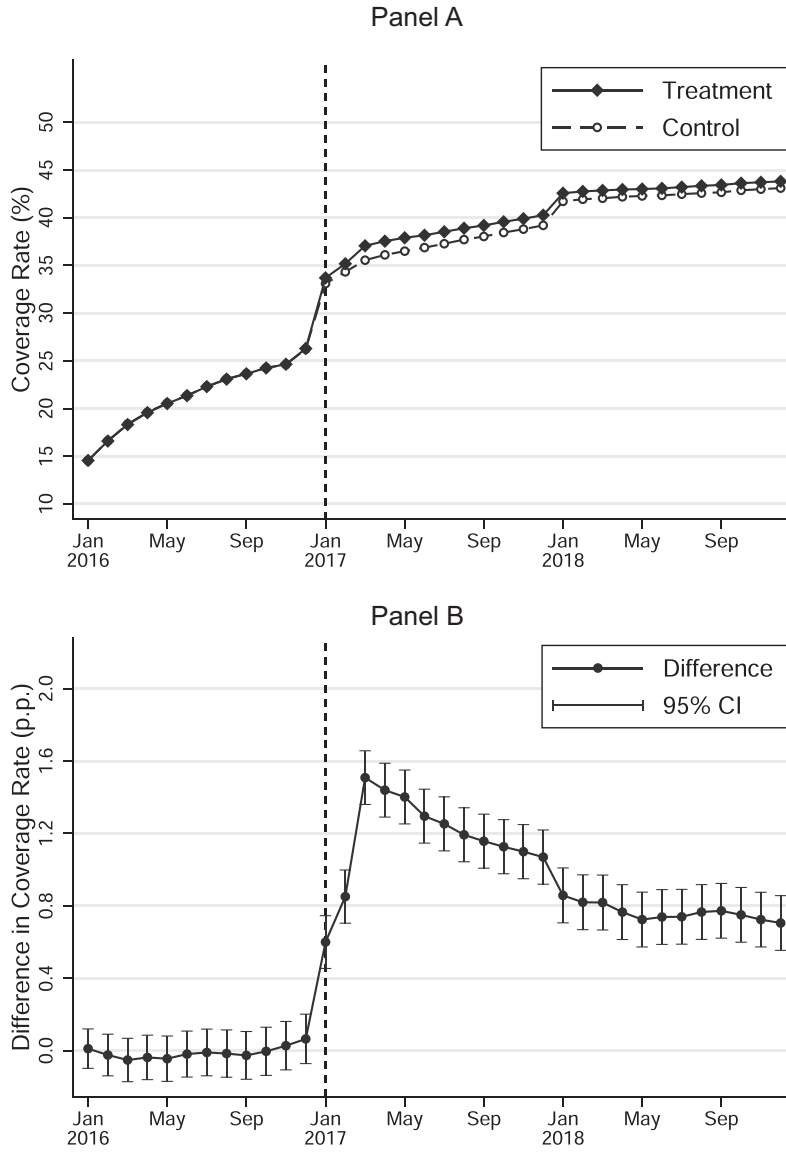


FIGURE II

Effect of Intervention on Coverage

Panel A displays the shares of the treatment and control group enrolled in any coverage in the specified month. Panel B displays the difference in the share between the treatment and control groups enrolled in any coverage in the specified month. Units are percentage points (0–100). Both panels exclude individuals with full coverage in January through November 2016. Brackets denote the 95% confidence interval based on standard errors that are clustered by household.

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design, the difference in coverage rates is approximately zero during 2016, before the intervention occurred. Coverage rates increase in January 2017 among individuals in both the treatment and control groups, but the increase for the treatment group is larger than for the control. The difference in coverage rates peaks at 1.51 percentage points in March 2017. Given the timing of the intervention and the rules for beginning new coverage (described in Section II), this timing is consistent with most individuals signing up for coverage just before the open enrollment deadline and having March as their first effective month of Exchange coverage. After March, we observe a gradual decline in the treatment effect over the remainder of the sample period. The decline may be due to individuals dropping coverage they initially enrolled in because of the intervention (e.g., by failing to make monthly payments) as well as greater scope for new enrollment by the control group in the latter portion of the outcome period. Consistent with the latter possibility, the largest month-to-month decline in the treatment effect occurs between December 2017 and January 2018, when new enrollments for 2018 coverage would begin to take effect. Consistent with the decline being at least partially due to individuals dropping coverage, Online Appendix Figure A.IV shows that the treatment effect attrition appears to be driven by gradual declines in Exchange coverage; we observe a smaller decline over the course of 2017 in the effect on Medicaid. In December 2018, the final month of our two-year outcome period, the difference in coverage rates among the treatment and control groups was just over half as large as the peak difference and remained statistically significant.[21]

As described in Section II, taxpayers could avoid the individual penalty by enrolling in a range of coverage types. Table III investigates the form of coverage-months induced by the intervention. We observe the largest effect on Exchange coverage; on average, the intervention increased enrollment in Exchange plans by 0.14 months during the outcome period, a 20% increase relative to the control group mean of 0.70 months.

21. To the extent that the additional coverage induced by the intervention reduced mortality (as we explore in Section V), the intervention could mechanically increase coverage in later months of the outcome period by increasing the share of the treatment group that is alive at that time to enroll in coverage. Online Appendix B.1 formalizes this possibility and estimates that no more than 0.4% of the observed coverage effect operates through this channel. For middle-aged adults, we estimate that no more than 1.1% of the coverage effect operates through this channel.

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TABLE III
COVERAGE EFFECT BY TYPE OF COVERAGE

	Exchange (1)	Medicaid (2)	ESI (3)	Off-Exchange (4)	VA (5)	Medicare (6)
Panel A: All ages						
Treated	0.141	0.071	0.024	0.005	0.005	0.003
	(0.006)	(0.012)	(0.014)	(0.002)	(0.001)	(0.002)
Control mean	0.696	3.367	5.341	0.149	0.046	0.178
Observations	5,084,165	5,084,165	5,084,165	5,084,165	5,084,165	5,084,165
Panel B: Middle-aged adults						
Treated	0.272	0.069	0.014	0.009	0.011	0.001
	(0.012)	(0.015)	(0.022)	(0.004)	(0.003)	(0.005)
Control mean	1.030	1.955	4.507	0.126	0.073	0.280
Observations	1,358,983	1,358,983	1,358,983	1,358,983	1,358,983	1,358,983

Notes. The table reports the effect of the intervention on specific forms of health insurance coverage. The outcome is the number of months of the specified form of coverage enrolled in during 2017–2018. ESI refers to employer-sponsored coverage. Off-Exchange refers to individual coverage not purchased through the Exchange. VA refers to coverage provided through the Veterans Administration. Panel B limits the analysis to individuals between the ages of 45 and 64 at the end of 2017. All columns exclude individuals with full coverage in January through November 2016. Standard errors, reported in parentheses, are clustered by household.

The effect on Medicaid coverage was approximately one-half as large in absolute terms, and smaller still when expressed as a percentage of the control group mean. We find very small effects of the intervention on ESI, off-Exchange individual coverage, coverage provided by the Veterans Administration, and Medicare. We observe similar magnitude effects among the middle-aged sample, with the exception of Exchange and individual off-market coverage, for which the estimated coefficients are nearly twice as large as those estimated from the overall population. Qualitatively, we observe similar patterns when we investigate the effect of the intervention on the likelihood of having any of a specified form of coverage during the outcome period (Online Appendix Table A.VII), although with respect to that outcome we do observe a nonzero (approximately 0.16 percentage point) increase in the probability of having any ESI during the outcome period.[22]

Online Appendix Table A.VIII considers heterogeneity in the coverage effect by household income and whether the taxpayer's state chose to expand Medicaid under the ACA. In states that

22. A factor that could help explain this difference is that unlike the Exchange and Medicaid coverage effects, the effect on ESI does not materialize until 2018 (Online Appendix Figure A.IV). This discrepancy may be due to the letters being received too late for most employees to participate in their employers' open enrollment windows for 2017 coverage.

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expanded Medicaid eligibility, households with incomes less than 138% of the federal poverty line typically qualify for coverage under the program. Expansion and nonexpansion states may also differ in terms of the generosity of plans offered through their respective exchanges (e.g., Clemens 2015). Across all states, we observe larger effects of the intervention on coverage among individuals whose household income fell below the Medicaid threshold, and a larger effect in states that expanded Medicaid than in those that did not. Online Appendix Table A.IX shows that the larger effect in expansion states appears driven by new Medicaid enrollment, primarily (but not exclusively) among individuals with prior-year incomes below the Medicaid eligibility threshold. In contrast, we find no evidence that the intervention increased Medicaid in the nonexpansion states. Unlike with Medicaid, the intervention increased enrollment into other forms of coverage in both expansion and nonexpansion states (Online Appendix Table A.X). Interestingly, the effect of the intervention on non-Medicaid coverage among low-income households was larger in the nonexpansion states than in the expansion states, consistent with the possibility that some of those induced to enroll in Exchange coverage would have instead enrolled in Medicaid had it been available.

Online Appendix Table A.XI explores other sources of heterogeneity in the effect of the intervention on coverage. We observe similar effect sizes for men and women (columns (1) and (2)), but moderately larger effects for married compared to unmarried taxpayers (columns (3) and (4)). We observe smaller effects for individuals who self-prepared their returns relative to those who used professional assistance (columns (5) and (6)), which may reflect differences in tax knowledge and awareness of the penalty. Finally, we observe larger effects of the treatment in states that had more successful initial rollouts of their Exchange websites compared to states that initially experienced a greater amount of technical difficulty (columns (7) and (8)).[23]

Until this point we have analyzed the effect of the intervention on coverage by pooling across the intervention's various treatment arms. Online Appendix Table A.XIII presents results separately by treatment arm. The base treatment, which was

23. Online Appendix Table A.XII reveals similar patterns of heterogeneous treatment effects when these factors are considered jointly within a single regression and when the outcome is defined as enrolling in any coverage during the outcome period.

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sent near the close of open enrollment for 2017 and contained a personalized estimate of the taxpayer's 2017 penalty amount, increased coverage by an average of 0.23 months. The nonpersonalized treatment, which provided a general formula for the taxpayer's 2017 penalty amount, was approximately 26% less effective than the base treatment, increasing enrollment by 0.172 months on average relative to the control. In contrast, the early treatment, which was sent near the start of 2017 open enrollment, was approximately 40% more effective than the base treatment, increasing enrollment by an average of 0.327 months relative to the control. Including information about claiming an exemption did not alter the effectiveness of the intervention, nor did providing a Spanish translation of the letter's contents. We observe similar patterns when assessing the effect of the intervention on the likelihood of enrolling in any coverage during 2017–2018 and when we restrict the analysis to middle-aged adults.

V. Mortality Effect

In this section, we exploit the exogenous variation in health insurance coverage induced by the intervention to better understand the relationship between coverage and mortality. Section V.A estimates the causal effect of the intervention on mortality. Section V.B considers the conditions under which we can attribute the observed reduction in mortality to the outreach-induced coverage and discusses potential mechanisms through which the new coverage may have reduced mortality. Section V.C uses an instrumental variable (IV) analysis to estimate the magnitude of the effect of coverage on mortality. Section V.D considers extrapolating the IV estimand to other parameters that may be of interest for policy. Section V.E compares our results to prior findings in the literature.

To increase statistical power, we restrict most of our analyses in this section to previously uninsured adults between the ages of 45 and 64—a group for whom death is less rare compared with younger individuals and for whom the effect of the intervention on coverage is relatively large (see Online Appendix Figure A.V).[24] As in the prior section, we focus on individuals who

24. Studies of health insurance on mortality typically restrict the sample population to older adults, but the specific age range varies by study (compare Baker et al. 2006; Sommers, Baicker, and Epstein 2012; Khatana et al. 2019; Miller et al. 2019). In our setting, the age range we consider shapes the power of

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lacked coverage at some point during the previous year because the effects of the intervention on coverage appear to be limited to this group. We provide summary statistics for this population in Online Appendix Table A.XIV and consider the robustness of our results to other populations below.

V.A. The Effect of the Intervention on Mortality

This subsection investigates the causal effect of the intervention on mortality. Figure III, Panel A presents the cumulative mortality rate over time for the treatment and control groups, and Panel B plots the difference in the cumulative mortality rates of these groups over time. The mortality rates for the two groups appear similar during 2016 but diverge over the two years following the intervention.[25] Because the treatment was randomly assigned, the figure provides visual evidence that the intervention reduced mortality.[26]

Table IV, column (1) presents the intent-to-treat effect of the intervention on 2017–2018 mortality. The cumulative mortality rate for the control group during this period was approximately 1%. Comparing the mean mortality rate between the treatment and control groups implies that the intervention reduced mortality by 6.3 basis points during the outcome period, or 1 fewer death for every 1,587 treated individuals. The p-value associated with this estimate is approximately .01; a permutation test yields

our analysis by affecting the strength of the first stage (which is increasing in the minimum age included), the baseline mortality rate (which is increasing in the minimum age included), and the sample size (which is decreasing in the minimum age included). To consider these factors together, Online Appendix Figure A.V presents results from simulations of the effect of the intervention on mortality under a range of assumptions about the magnitude of the mortality effect and the baseline mortality of the compliers. For 8 of the 12 combinations of parameter values we consider, the 45–64-year-old age range maximizes the likelihood of detecting an effect of the treatment on mortality when such an effect is present.

25. Because the sample was constructed to exclude individuals whose deaths had been recorded by July 2016, the mortality rate in both the treatment and control groups is lower during the first half of 2016 than in subsequent months. As described in Section II, the observed mortality rate for the first half of 2016 was not zero because some deaths that occurred during this period were first recorded in our data after the sample was already constructed and because we did not exclude returns that listed dependents who died prior to the date of sample construction.

26. The same pattern is also present (but noisier) in Online Appendix Figure A.VI, which plots the difference in deaths in each six-month period (rather than the difference in cumulative mortality) over time by treatment assignment.

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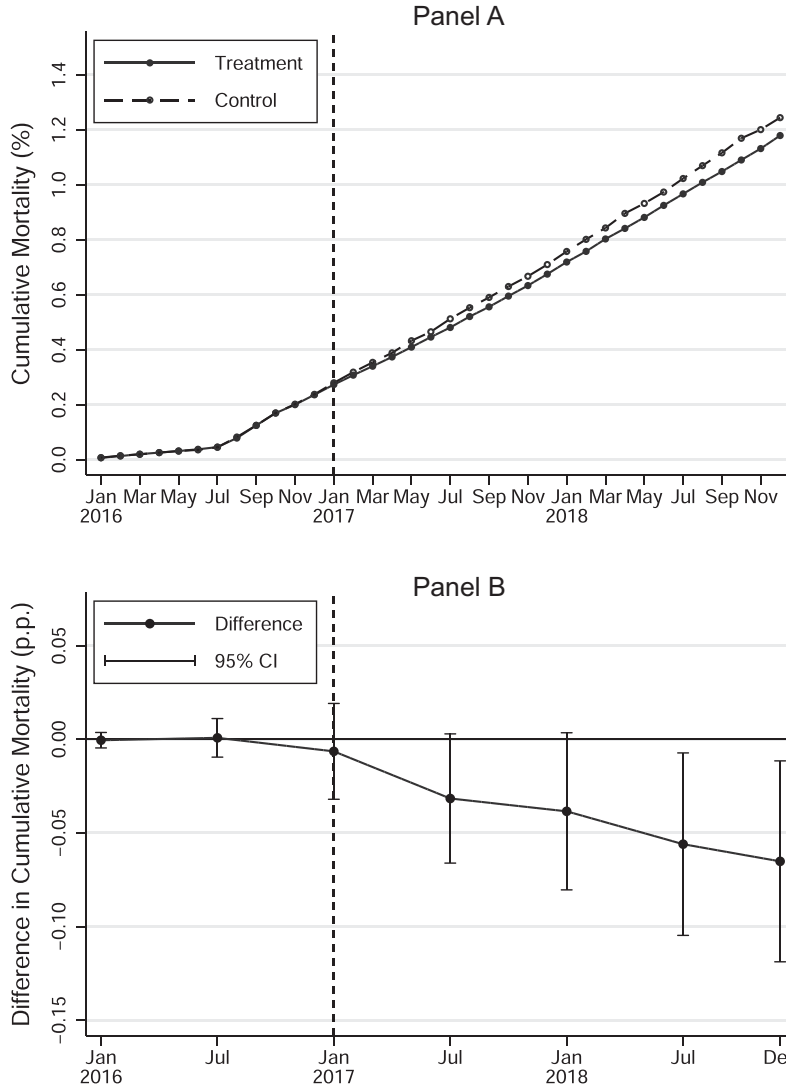


FIGURE III

Effect of Intervention on Middle Age Mortality

Panel A displays the share of middle-aged adults that died during or prior to the specified month. Panel B displays the difference in the cumulative mortality rate among middle-aged adults in the control and treatment groups. The difference is calculated at six-month intervals that extend through the end of the specified month. Units are percentage points (0–100). Both panels are limited to individuals between the ages of 45–64 at the end of 2017 and exclude individuals with full coverage in January through November 2016. Brackets denote the 95% confidence interval based on standard errors that are clustered by household.

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TABLE IV
EFFECTS OF INTERVENTION AND COVERAGE ON MIDDLE-AGE MORTALITY

	Mortality (reduced form) (1)	Mortality (OLS) (2)	Coverage (first stage) (3)	Mortality (IV) (4)
Treated	−0.063		0.358	
	(0.025)		(0.026)	
Covered months		−0.026		−0.178
		(0.001)		(0.070)
Control mean	1.007	1.007	7.795	1.007
Observations	1,358,983	1,358,983	1,358,983	1,358,983

Notes. The table reports analyses relating to the effect of the intervention on mortality and to the effect of coverage on mortality. In columns (1), (2), and (4), the outcome indicates mortality during 2017–2018; units are percentage points (0–100). In column (3), the outcome is months of coverage during 2017–2018. Column (1) reports the intent-to-treat effect of the intervention on mortality. Column (2) reports results from an OLS regression of mortality on 2017–2018 coverage-months. Column (3) reports the first stage for the IV estimate; the effect of the intervention on months of coverage during 2017–2018. Column (4) reports the effect of coverage on mortality obtained by instrumenting for months of 2017–2018 coverage with an indicator for treatment group assignment. All columns limit the analysis to individuals between the ages of 45 and 64 at the end of 2017 and exclude individuals with full coverage in January through November 2016. Standard errors, reported in parentheses, are clustered by household.

similar results (Online Appendix Figure A.VII). Note that this result speaks to the average number of life-years saved during the outcome period but not to their distribution; that is, we cannot separately identify how many lives were extended versus the average extension length.[27]

We next consider several robustness checks relating to sample selection and specification. Online Appendix Table A.XV shows that the presence (but not the magnitude) of the mortality effect is reasonably robust to adopting alternative age cutoffs for defining the sample. Online Appendix Table A.XVI includes in the analysis individuals who were already fully insured in the year before the intervention and therefore lacked a substantial first-stage effect. As expected, the estimated effect of the intervention is attenuated for this sample but remains statistically significant. Finally, Online Appendix Table A.XVII shows that the mortality effect is not sensitive to controlling for prerandomization variables or estimating a nonlinear limited dependent variable or

27. For example, the following two cases would contribute equally to our observed effect: (i) the intervention causes one person to die on 1/1/2019 instead of 1/1/2017; and (ii) the intervention causes one person to die on 1/1/2018 instead of 1/1/2017 and a second person to die on 1/1/2019 instead of 1/1/2018.

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duration model. With controls, the point estimate of the mortality effect from the linear dependent variable model increases slightly in magnitude (columns (1) and (2)). Modeling mortality using a logit model yields a marginal effect of the intervention on mortality of 5.2 basis points with controls and 6.3 basis points without controls (columns (3) and (4)). Both a proportional hazard model and a log-rank test reject the null hypothesis of equality between the treatment and control group survival curves (columns (5)–(7)).

To provide additional context for interpreting the effect of the intervention on mortality, we estimate the budgetary cost per life extended through the two-year period following the intervention. The budgetary cost of the intervention includes the direct mailing and postage costs of the letters (approximately $0.49 per treated household, or $0.25 per treated individual) as well as indirect federal and state budgetary costs from forgone tax revenue and subsidies for newly induced coverage. In contrast, the analysis does not account for social costs of health insurance other than those borne by the government or for the potential benefits of health insurance other than reductions in short-term mortality. Online Appendix Table A.XVIII estimates the indirect costs of the intervention to be on the order of $151.65 per treated individual for middle-aged adults who lack prior-year insurance, implying a combined direct and indirect budgetary cost of roughly $241,111 per life extended through the two-year window we observe.[28]

V.B. *Did the Intervention Reduce Mortality by Increasing Coverage?*

The intent-to-treat estimate reported in Section V.A suggests that the intervention causally reduced mortality. Attributing

28. The indirect cost estimate reported in Online Appendix Table A.XVIII accounts for the effect of the intervention on budgetary costs arising from premium tax credits claimed; cost-sharing reductions; federal and state costs of Medicaid coverage; forgone federal tax revenue from reductions in the individual mandate penalty that were paid; and forgone federal and state tax revenue from income tax exclusions for employer-provided health insurance. The estimate does not account for other potential labor market effects of the letters on federal and state tax revenues, such as unemployment, disability, or retirement, or for nonbudgetary social costs such as increased health spending that result from the intervention. The sum of direct and indirect budgetary costs per treated individual is equal to $0.25 (direct) + $151.65 (indirect), or $151.90. The total budgetary cost per life extended is therefore given by ($151.90 per treated individual) / (0.00063 fewer deaths per treated individual), or $241,111 per life extended.

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the mortality reduction to health insurance requires ruling out the possibility that the intervention affected mortality through other channels. With the addition of this exclusion restriction, our results imply that the coverage induced by the intervention reduced taxpayers' mortality.

We conduct two placebo tests to assess the validity of the assumptions that underlie this interpretation of our results. The first investigates the effect of the intervention on 2016 mortality (before the intervention occurred) to assess the possibility that the observed mortality effect reflects changes in the reporting of mortality or preexisting health differences between the treatment and control groups. Online Appendix Table A.XIX, column (1) shows that the estimated effect of the intervention on 2016 mortality is near zero and not statistically significant. The second test investigates the effect of the intervention on mortality for a subset of the population for which the intervention was significantly less likely to induce new coverage: those who were enrolled in full-year coverage during 2016. If the intervention affected mortality through a channel other than inducing new coverage (in violation of the exclusion restriction), we might observe its effect for this group as well. Instead, we estimate the effect of the intervention on mortality for those with full-year coverage during 2016 to be much smaller (approximately 1 basis point) and not statistically significant (column (2) of Online Appendix Table A.XIX). Online Appendix Figure A.VIII provides visual evidence consistent with the lack of a mortality effect for this group.

To further investigate whether the observed reduction in mortality was driven by increases in coverage, we next assess how differences in the first-stage coverage effect among various groups in the sample relate to group-level differences in the reduced-form effect of the intervention on mortality. Online Appendix Figure A.IX plots estimated coverage and mortality effects separately by treatment arm. The slope of the best-fit line is negative (as predicted by the hypothesis that the mortality effects are due to changes in coverage), but the differences in the estimated mortality effects across treatment groups are not jointly significant (Online Appendix Table A.XX). In similar spirit, Online Appendix Figure A.X presents variation in the coverage and mortality effects by state. As with the previous analysis, the best-fit line is downward sloping but the relationship is imprecisely estimated. Finally, Figure IV compares estimated coverage and mortality effects by combining variation arising from treatment arm, state of

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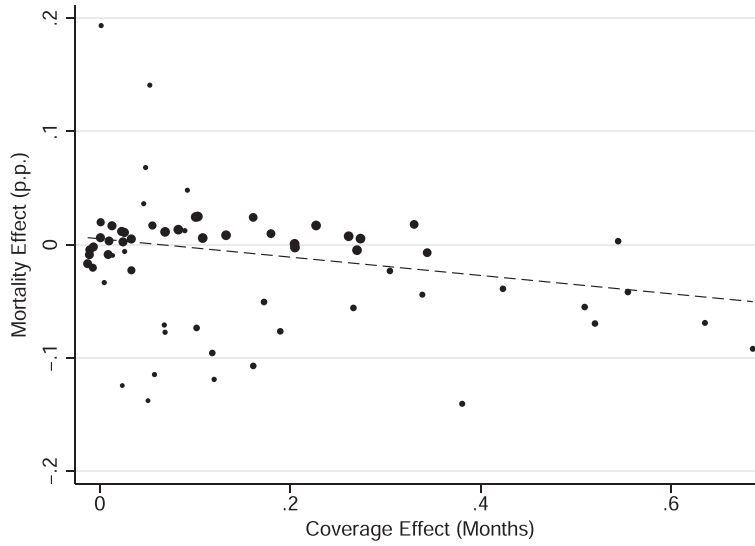


FIGURE IV

Coverage and Mortality Effects by Treatment Arm and Individual Characteristics

The figure plots the estimated effect of the intervention on coverage and mortality, separately for groups of individuals defined based on treatment arm, state of residence, age, and prior-year coverage. With respect to state of residence, individuals are grouped according to whether the estimated coverage effect for their state was above or below the median state's estimated coverage effect. With respect to age, individuals are grouped according to whether they are aged 45–64 at the end of 2017; individuals aged older than 64 are excluded from the analysis. With respect to prior year coverage, individuals are grouped according to whether they were enrolled in full coverage in January through November 2016. The coverage effect (x-axis) corresponds to the effect of the intervention on the number of months of coverage enrolled in during 2017–2018. The mortality effect (y-axis) corresponds to the effect of the intervention on mortality in 2017–2018; units are percentage points (0–100). The best linear fit (dashed line), weighted by cell sample size, has a slope of −0.081, with standard error 0.022.

residence, age, and prior-year insurance status. The figure shows a downward-sloping and statistically significant relationship, although we caution that differences across individual cells may conflate heterogeneity in the intensity of the coverage effect with heterogeneity in the effect of coverage on mortality.[29]

29. In particular, treatment arms may differ in the composition of individuals they induce to enroll in coverage; states may differ in the composition of their populations and in the form of coverage induced; and treatment effects may differ across individual-level characteristics.

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Although these tests provide some support to the exclusion restriction, there are a number of conceivable channels through which the assumption could be violated. First, it could be that by reducing the share of taxpayers owing a penalty, the intervention reduced mortality by increasing after-tax incomes. However, Online Appendix Table A.XIX, column (3) shows that the intervention reduced penalties by an average of only $4.70 per year—too small an amount to account for the observed mortality effect.[30] Second, the intervention may have affected mortality through changes in labor supply from taxpayers entering the labor market or switching jobs to obtain ESI. However, Online Appendix Table A.XIX, columns (4) and (5) show that the intervention did not lead to a substantial change in ESI until 2018, by which point the observed mortality reduction had already begun to occur (column (6)). Third, the intervention may have caused individuals to adjust downward their beliefs about plan costs, reducing mortality by reducing stress about the ability to purchase coverage in the future. Although chronic stress may affect health, many of the channels through which it is hypothesized to do so are longer term, such as atherosclerosis (Yao et al. 2019), hypertension (Bhelkar et al. 2018), or gradual weakening of the immune system (Segerstrom and Miller 2004). Fourth, some of the low-income individuals induced to apply for coverage may have been subsequently recruited to participate in safety net programs like the Supplemental Nutrition Assistance Program or Temporary Assistance for Needy Families, and it could have been those programs, rather than the new coverage itself, that reduced their mortality. Yet our finding (discussed below) that the mortality effect does not appear limited to households below the Medicaid income threshold provides evidence against this

30. For example, using the cross-sectional relationship between 2016 household income and two-year mortality among those in the control group would imply that the change in penalty liability reduced the mortality rate by only 6.27×10^{-7}, well outside our estimated 95% confidence interval for the effect of the intervention on mortality. A related possibility is that the observed mortality reduction is driven by a reduction in disposable income caused by taxpayers' additional spending on health insurance premiums. Such spending could reduce short-term mortality by displacing consumption of drugs or alcohol (Dobkin and Puller 2007) or by tempering other forms of consumption or activity (Evans and Moore 2011; Andersson, Lundborg, and Vikström 2015). These studies find that the mortality-inducing effects of income are strongest among the young, but, as described below, we do not observe a mortality reduction for that group.

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hypothesis. Finally, because our intervention does not provide either new coverage or new discounts on existing coverage, our design avoids a potential concern that is present in lottery-based evaluations, that is, that the effect of coverage on health outcomes is conflated with the psychological effect of "winning" a lottery to obtain coverage (a possibility discussed in Finkelstein et al. 2012).

We turn from violations of the exclusion restriction to potential channels through which the coverage induced by the intervention may have reduced mortality. Although we lack data on cause of death, the timing of the observed mortality effect can narrow the range of possibilities. In particular, for coverage to reduce mortality over the one- to two-year time horizon we observe, it must affect conditions that (i) can cause death quickly if left untreated or unmanaged, and (ii) for which treatment or management can prevent or delay mortality. In contrast, coverage would not be expected to drive such rapid mortality effects by inducing diagnosis of chronic conditions that are susceptible to early but not late-stage treatment, such as certain forms of cancer (Sommers, Long, and Baicker 2014).

One mechanism consistent with these conditions is that for acute, life-threatening conditions (e.g., heart attack or stroke), the new coverage reduced mortality by reducing delays between the onset of symptoms and the start of medical treatment. Uninsured people may delay seeking emergency treatment for such conditions because they do not realize the severity of the health risk and expect hospitals to refuse to treat them or wish to avoid the medical bills they expect treatment to entail (Smolderen et al. 2010; Medford-Davis et al. 2016). Moreover, such delays can significantly increase the likelihood of near-term mortality (Moser et al. 2006). A related possibility is that insurance may improve health outcomes by causing medical providers to provide, or patients to choose to receive, more extensive treatment during emergency visits or hospitalizations (Doyle 2005; Card, Dobkin, and Maestas 2009). Consistent with these explanations, in our heterogeneity analysis we observe the largest mortality point estimate among individuals with incomes too high to retroactively enroll in Medicaid if treated.

A different channel through which intervention-induced coverage may have reduced mortality over our outcome period is by leading to improved diagnosis and treatment of subacute conditions with relatively high near-term mortality. For example, Wherry and Miller (2016) find large and positive effects of new

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coverage on diagnoses of high cholesterol, and cardiovascular drugs have been observed to reduce mortality from heart disease within months of beginning treatment (Aronow et al. 2001; Cannon et al. 2004).[31] Health insurance could similarly reduce mortality among individuals who have already been diagnosed with a chronic condition, such as by improving access to primary care physicians or reducing the need to skip medications due to cost (Sommers et al. 2017b).

Finally, it is possible that the intervention-induced coverage reduced mortality by lowering stress on the part of new enrollees or family members, either by insuring them against financial shocks or through a more general "peace of mind" effect (Haushofer et al. 2020). Notably, to the extent coverage reduced mortality through this channel, the effect would not be limited to those who consumed additional health services because of the new coverage. On the other hand, as discussed already, we would expect most of the health effects from changes in stress to manifest outside of our two-year outcome period.

V.C. The Average Effect of Outreach-Induced Coverage on Mortality

Sections V.A and V.B provide evidence that the coverage induced by the intervention reduced mortality but do not directly speak to the magnitude of this effect. Here we exploit treatment group assignment as an instrument for the number of coverage-months in which one enrolls during the outcome period. Under two additional assumptions, this analysis yields the average effect of the coverage induced by the intervention on mortality.

The first additional assumption is that the effect of the intervention on coverage is weakly monotonic: each individual enrolls in at least as many months of coverage when assigned to the treatment as when assigned to the control. This assumption would be violated, for example, if some individuals were "nudge averse," so that receiving the IRS letter caused them to enroll in less coverage than they would otherwise obtain. Although monotonicity is

31. Baicker et al. (2013) also report large and positive (but statistically insignificant) estimates for the effect of Medicaid coverage on high-cholesterol diagnoses. Along similar lines, Khatana et al. (2019) report reductions in mortality from cardiovascular disease in the initial years following state Medicaid expansions. Coverage could also reduce mortality by inducing other forms of preventative care, such as influenza vaccination (Lu, O'Halloran, and Williams 2015).

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not directly verifiable, a necessary condition for it to hold in our setting is that the cumulative distribution functions of coverage-months for the treatment and control groups do not cross (Angrist and Imbens 1995). Online Appendix Figure A.XI establishes that this condition is satisfied in our data. In addition, because monotonicity must hold for each individual, the assumption also implies that the cumulative distributions of coverage-months should not cross among any subset of the sample (at least in expectation). Online Appendix Figure A.XII presents evidence consistent with this hypothesis across various demographic cuts.

In addition to monotonicity, the IV analysis requires an assumption about the margin of coverage through which the intervention affected mortality. In particular, we assume the intervention affected mortality only by changing the number of coverage-months in which individuals enrolled during the outcome period. Because health insurance enrollment occurs at the monthly level (i.e., an individual's coverage status cannot generally vary within a calendar month), this strengthening of the exclusion restriction strikes us as plausible. However, the assumption could be violated if the intervention affected individuals' coverage along other margins, such as by inducing individuals to switch from one form of coverage to another or to enroll in more generous insurance plans. If the intervention caused such changes in behavior and those changes contributed to the observed mortality reduction, our IV estimate will be biased. Although we cannot rule out such possibilities, we view them as unlikely. For example, we do not observe evidence that the intervention reduced enrollment in any particular type of coverage (see Table III), as might have occurred if the intervention caused individuals to systematically switch between coverage types. Similarly, we observe no evidence of a change in plan quality among those who were likely to enroll in coverage absent the intervention, relying on pre-subsidy plan premiums as a proxy for quality (Online Appendix Table A.XXI).

Another possibility that could bias the IV estimate upward is if the new coverage reduced mortality for others in the household, including those who did not enroll in additional coverage of their own (Borgschulte and Vogler 2020). Online Appendix Table A.XXII investigates this possibility by focusing on households containing at least one person with full-year 2016 coverage and another person without full-year 2016 coverage. To investigate spillover effects from the intervention-induced coverage, we

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examine the mortality effect of the intervention among the household members with full prior-year coverage. Because the intervention did not increase coverage for this group (column (1)), but did increase coverage for their household members (column (2)), observing a mortality effect would indicate the presence of within-household spillovers. However, the estimated mortality effect for this group (column (3)) is near zero, providing no evidence for such spillovers.

Before turning to the IV results, Table IV, column (2) presents benchmark results from an OLS regression of mortality on coverage. Each additional month of coverage is associated with a small (2.6 basis point) but statistically significant reduction in the probability of death during the outcome period. Because the OLS estimate is identified from cross-sectional variation in coverage, however, it may conflate the causal effect of coverage on mortality with differences in the health of those who enroll.

Turning to our main IV specification, we instrument for the total number of coverage-months in which an individual enrolled during the outcome period, using as our instrument an indicator for whether the individual was assigned to the treatment group.[32] Scaling the estimated effect of the intervention on mortality (reported in Table IV, column (1)) by the estimated effect on coverage-months (column (3)) yields an IV estimate of approximately −0.18 percentage points (column (4)). Under the assumptions described above, this analysis identifies the average causal response (ACR) of mortality to coverage, that is, the per-month effect of coverage on mortality, averaged over the additional months of coverage that individuals enrolled in because of the intervention (Angrist and Imbens 1995). The 95% confidence interval for the IV estimate extends from −0.31 to −0.04 percentage points, encompassing both moderate and very large values. Notably, the OLS and IV confidence intervals do not overlap. This discrepancy

32. A different approach would be to instrument for whether an individual enrolled in any coverage during the outcome period. However, this "extensive-margin" IV requires a stronger exclusion restriction than our preferred approach; it is unbiased only when (i) coverage-months beyond the first yield no incremental effect on mortality, or (ii) the intervention only increases coverage among individuals who, without the intervention, would have enrolled in zero months of coverage during the outcome period (Andresen and Huber 2020). We view the latter condition as implausible in our setting; as discussed in Section IV, the intervention likely caused some individuals to enroll in 2017 coverage who, without the intervention, would have enrolled in positive months of coverage during 2018. We consider the implications of imposing the former condition in Section V.D.

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may be due to nonrandom selection into coverage; under adverse selection, for example, the OLS estimate of the effect of coverage on mortality would be upward-biased because those who enroll in coverage tend to have worse health than those who do not. Alternatively, the difference between the IV and OLS results could reflect differences in the composition of the populations from which the estimates are identified; for example, those who enroll in additional coverage because of the intervention may tend to benefit more from coverage than others in the population.[33]

Online Appendix Table A.XXIII provides a range of robustness checks for the IV analysis. Columns (1) and (2) obtain similar results as the main specification when controlling for a range of demographic and geographic variables or for randomization strata indicators. Columns (3) and (4) present results using two alternative age ranges: 40–64-year-olds—which yields an attenuated but still statistically significant effect of −0.12 percentage points—and 50–64-year-olds—which yields a similar estimate as our main specification. Columns (5) and (6) present results for the full population of 45–64-year-olds, including those who had already obtained full-year coverage by 2016. Column (5) pools the results across individuals with and without full prior-year coverage, and column (6) includes as a separate instrument an interaction of treatment group assignment with an indicator for lacking full year coverage in 2016 to account for the observed coverage effect heterogeneity across this dimension. Both approaches yield similar effects as our main specification. Finally, column (7) reports the results of a specification that instruments for coverage using separate binary indicators for each of the eight treatment arms rather than a pooled indicator for assignment into any one of the treatment groups. With this specification, the estimated effect of coverage on mortality declines slightly in magnitude (to approximately −0.14 percentage points) but remains statistically significant.

We next explore heterogeneity in the effect of coverage on mortality. First, we investigate heterogeneity by age. Figure V provides evidence that the coverage induced by the intervention

33. This possibility make it difficult to interpret the magnitude of the IV estimate in percentage terms. In particular, the mortality rate among the control group (approximately 1% in our data) may substantially understate the baseline mortality risk among those who enrolled in additional coverage because of the intervention (as discussed in Miller et al. 2019). We present suggestive evidence along these lines in Section V.D.

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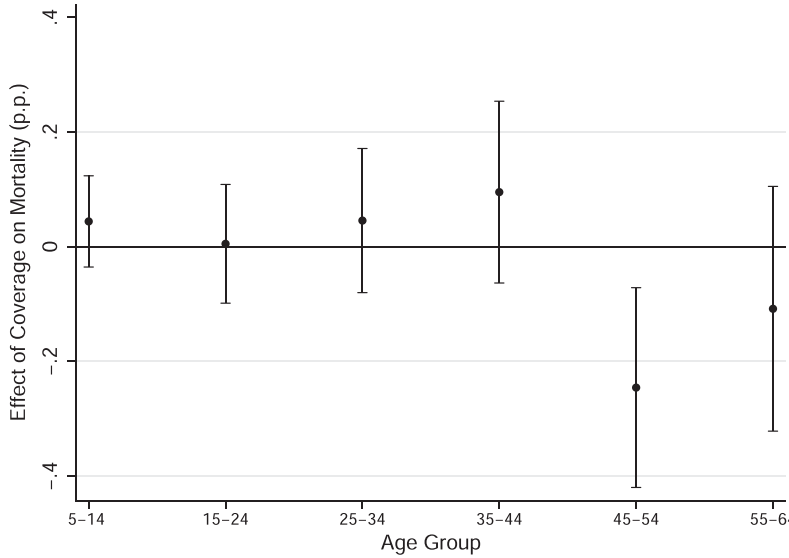


FIGURE V

Effect of Coverage on Mortality by Age

The figure displays the estimated effect of coverage on mortality by age. Each estimate is obtained from an IV specification in which an indicator for treatment group assignment is used to instrument for months of coverage during 2017–2018. The outcome is mortality in 2017–2018; units are percentage points (0–100). Each specification is limited to individuals whose ages fell into the specified range at the end of 2017. The figure excludes individuals with full coverage in January through November 2016. Brackets denote the 95% confidence interval based on standard errors that are clustered by household.

reduced mortality even among the younger group of middle-aged adults we consider (45–54-year-olds). The point estimate for 55–64-year-olds is also negative, but smaller in magnitude (although the difference is not statistically significant). In contrast, we observe no reduction in mortality among age groups younger than 45, consistent with the prior quasi-experimental findings reported in Sommers, Baicker, and Epstein (2012) and Miller et al. (2019).

Online Appendix Table A.XXIV presents additional exploratory heterogeneity analyses regarding the effect of coverage on mortality. Columns (1) and (2) report estimates separately by whether 2015 household income was above or below the 138% federal poverty level cutoff for Medicaid eligibility. The estimated mortality effect of coverage is negative among both income groups, but larger in magnitude among those above the Medicaid

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eligibility cutoff (although the difference is not statistically significant). Columns (3) and (4) report estimates separately for Medicaid expansion and nonexpansion states. The estimated effect in nonexpansion states is approximately twice as large in magnitude as the effect in expansion states, although again the difference is not statistically significant. We also failed to detect statistically significant differences in the effect of coverage on mortality based on gender (columns (5) and (6)) or marital status (columns (7) and (8)). Finally, in unreported results, we investigated, but did not observe, heterogeneity in the effect of coverage on mortality based on tax preparation method, the success of the state's initial marketplace rollout, regional variation in whether taxpayers reside in urban versus rural areas, or characteristics of health provider markets, such as the geographic concentration of primary care physicians or hospital emergency departments.

V.D. Extrapolation to Other Policy-Relevant Parameters

As discussed already, the IV analysis in Section V.C identifies the ACR of mortality to coverage—that is, the average mortality reduction per month of coverage induced by the intervention. The ACR is most directly relevant for assessing the mortality effects of outreach policies like the one we study. In this subsection, we consider how our results might inform additional policy-relevant parameters, such as the effect of annual coverage or the effect of extending coverage to other parts of the uninsured population.

To shed light on these issues, following Angrist and Imbens (1995), we can express the ACR as

$$(1) \qquad ACR = \sum_{m=1}^{24} w_m \, E\left[Y_i(m) - Y_i(m-1) \,|\, C_i(1) \geqslant m > C_i(0)\right],$$

where $Y_i(m) \in \{0, 1\}$ indicates whether individual i would die during the outcome period if i were to enroll in $m \in \{0, 1, \ldots, 24\}$ months of coverage; $C_i(0)$ and $C_i(1)$ indicate the months of coverage in which i would enroll if assigned to the control or treatment, respectively; and the weights w_m are given by

$$w_m = \frac{Pr\left(C_i(1) \geqslant m > C_i(0)\right)}{\sum_{j=1}^{24} Pr\left(C_i(1) \geqslant j > C_i(0)\right)}.$$

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Equation (1) highlights that the ACR is shaped by which individuals enrolled in more coverage because of the intervention and the distribution of coverage-months in which they enrolled. We next consider each of these elements in turn (beginning with the latter).

1. *The Effect of Annual Coverage on Mortality.* To understand how the ACR relates to the effect of annual coverage on mortality, we examine the degree to which the intervention induced an uneven distribution of new coverage-months. To do so, we estimate, for each $m \in \{1, 2, \ldots, 24\}$,

$$C_i^m = \kappa^m + \gamma^m Z_i + \varepsilon_i^m, \tag{2}$$

where $Z_i \in \{0, 1\}$ indicates whether i was assigned to the treatment group and where C_i^m indicates whether individual i attained at least m months of coverage, $C_i(Z_i) \geqslant m$. The γ^m coefficients identify the weights (w_m) from equation (1) that aggregate the per-month effects of coverage into the ACR. Online Appendix Figure A.XIII shows that the intervention induced more initial coverage-months than subsequent coverage-months; for example, 15.2% of the coverage-months added by the intervention constituted the first, second, or third month of coverage in which the individual was enrolled during 2017–2018, as compared with 6.7% of coverage-months that constituted the 22nd, 23rd, or 24th month of coverage for the individual during the same time period. This suggests the ACR is disproportionately weighted toward the mortality effect of initial coverage-months.

Because the intervention induced a nonuniform distribution of new coverage-months, recovering the effect of annual coverage from the ACR requires accounting for potential nonlinearities in the relationship between coverage and mortality. For example, if the relationship between coverage and mortality is concave, simply multiplying the ACR by 12 would overestimate the effect of annual coverage because the contribution of initial coverage-months would be weighted too heavily. Such concavity might arise, for example, if individuals obtain many of the health benefits of full-year coverage by fitting their consumption of health services into the months in which they do have coverage (Diamond et al. 2018). In contrast, if what matters for reducing mortality is having coverage at the onset of an unexpected acute condition, then the relationship between coverage-months and mortality may be closer to linear.

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To provide a sense for how concavity in the relationship between coverage-months and mortality shapes the interpretation of our estimates, suppose the mortality effect of an incremental month of coverage is homogeneous across individuals, so that we can express the ACR as:

$$ACR = \sum_{m=1}^{24} w_m \beta_m , \tag{3}$$

where $\beta_m = Y_i(m) - Y_i(m-1)$ for all i. Online Appendix Table A.XXV considers the implications of the β_m coefficients reflecting varying degrees of concavity, given the estimated distribution of intervention-induced coverage-months. Columns (1) and (2) report bounds for the effect of a full year of coverage, under the assumptions that the magnitude of the mortality effect from an incremental coverage-month is nonincreasing in the number of months of coverage enrolled in during the outcome period and that the effect of each coverage-month is nonpositive. We find that the magnitude of the effect is minimized when only the first two months of coverage have any effect (column (1)) and maximized when all months of coverage matter equally (column (2)). The corresponding parameter values imply that a full year of coverage would reduce mortality during the outcome period between 1.73 and 2.13 percentage points. Column (3) considers the limiting case in which only the first month of coverage affects mortality; under that assumption, the implied effect of a full year of coverage on mortality is also approximately 1.73 percentage points.[34] Note that the uncertainty reflected in these bounds is fairly small relative to the uncertainty from statistical imprecision in our estimate of the ACR itself. For example, applying this approach to the lower-magnitude end of the 95% confidence interval for the ACR implies a much smaller but still substantial mortality reduction from a year of coverage between 0.39 and 0.49 percentage points.

Although columns (1)–(3) shed light on the range of effects that are consistent with our data, point-identifying the effect

34. Under this assumption, the extensive-margin IV discussed in Section V.C identifies the average (cumulative) effect of the intervention-induced coverage among those who enrolled in nonzero coverage because of the intervention. Applied to our data, this approach implies that the intervention-induced coverage reduced mortality by an average of 3.47 percentage points among this group. However, because coverage-months other than the first likely affect mortality risk to at least some degree (Sudano and Baker 2003), we expect this estimate to be biased.

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of annual coverage on mortality requires additional structure. Toward that end, we next consider imposing that the magnitude of the monthly coverage effect declines in coverage-months at a geometric rate (see Online Appendix B.3 for details). To estimate the parameters of this model, we exploit variation in the distributions of coverage-months induced by the various treatment arms (Online Appendix Figure A.XIV). This exercise suggests that a year of coverage would reduce mortality during the outcome period by 1.91 percentage points—approximately 10% less than the reduction implied by assuming that mortality risk depends linearly on months of coverage.

2. *The Effect of Coverage on Mortality for Other Uninsured Groups.* We shift our focus from the composition of coverage-months induced by the intervention to the composition of individuals induced to enroll in additional coverage. Doing so sheds light on the implications of our results for policies that would extend coverage to broader portions of the uninsured middle-aged population.

Because the ACR is identified from the subset of individuals who increased their coverage in response to the intervention, it may not reflect the effect of coverage for the remainder of the uninsured population. For example, if those who responded to the intervention by enrolling in additional coverage were at higher mortality risk than those who did not, the mortality reduction from coverage for that group (and hence our estimated ACR) may be higher than for the rest of the uninsured middle-aged population.

One way to shed light on this hypothesis is to compare the baseline mortality rate (i.e., the mortality rate an individual would face without coverage) across those who did and did not enroll in additional coverage because of the intervention. Although additional assumptions would be required to identify this parameter for the overall complier population, the assumptions that permit identification of the ACR also allow us to identify the baseline mortality rate for the extensive-margin compliers—that is, the subset of individuals who would have enrolled in zero months of coverage but for the intervention (see Online Appendix B.2).[35] Following this approach, we estimate that absent coverage,

35. Extensive-margin compliers represent 1.85% of the middle-aged sample (Table II, Panel B, column (6)). As discussed above, it is unlikely that all compliers fall into this group; for example, the intervention likely caused some individuals to enroll in coverage starting in 2017 instead of 2018.

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extensive-margin compliers face a 1.8% mortality rate during the outcome period—over twice as high as the corresponding rate among those who did not obtain coverage following the intervention (Online Appendix Table A.XXVI). Although the baseline mortality rate for extensive-margin compliers may be higher or lower than for other complier groups, and although a difference in the baseline mortality rates is only one reason that the ACR may differ from the average effect of coverage on mortality for the overall uninsured population, this result suggests that the effect of coverage on mortality may be particularly large among the individuals induced into coverage from the intervention we study, as compared with other policies that reduce uninsurance.

Finally, the results in this subsection are useful for assessing the magnitude of our IV results. If one assumes that the extensive-margin compliers have a similar baseline mortality rate as others who increased their coverage in response to the intervention, our IV point estimate implies that on average, each month of coverage induced by the intervention during the outcome period reduced mortality by approximately 10.1%. This is a very large effect; the implied mortality reduction from a year of coverage (Online Appendix Table A.XXV) would be comparable to or larger than the overall baseline mortality rate, which we regard as implausible. We offer five potential (nonexclusive) explanations for this result. First, the magnitude of the IV point estimate may be too large due to statistical imprecision. The lower-magnitude bound of our 95% confidence interval for the effect of coverage implies an average per-month reduction in mortality of 2.2%, with a full year of coverage reducing mortality between approximately 22% and 27% of the baseline rate. Second, the baseline mortality rate of extensive-margin compliers may be less than the baseline mortality rate of others who increased their coverage in response to the intervention. Third, intervention-induced changes in coverage may have reduced mortality through margins that are not reflected in our month-based enrollment measure, such as through undetected changes in coverage type or plan generosity (in violation of the IV exclusion restriction) or spillovers to other household members. Fourth, some share of the population may have enrolled in less coverage because of the intervention (in violation of monotonicity); if such individuals benefit less from coverage than those who increased their coverage in response to the intervention, our estimated ACR would be biased upward. Finally, our IV estimate would be biased upward if the

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intervention reduced mortality through some channel other than inducing new coverage. Of these possibilities, we note that only the last could potentially invalidate our main qualitative conclusion that the intervention-induced coverage reduced mortality.

V.E. Comparison to Findings from Other Research

With respect to our mortality analysis, the previous study closest to ours in design is the Oregon Health Insurance Study (Finkelstein et al. 2012), which randomized access to Medicaid among a low-income population of applicants.[36] The Oregon study did not find a statistically significant effect of coverage on mortality; however, our 95% confidence interval substantially overlaps with theirs, with both including implied monthly effects of coverage on mortality between −0.04 and −0.10 percentage points (Online Appendix Table A.XXVII, columns (1) and (2)).[37] In addition, the average age in the Oregon study population was 41, compared with 53 among the middle-aged adults included in our mortality analysis. If the magnitude of the effect of coverage on mortality increases in age, as suggested by Figure V, it may be that differences in the age distribution of the two study populations contribute to the difference in point estimates. Indeed, reweighting the Oregon analysis to reflect the age distribution of our middle-aged sample increases the Oregon point estimate for the effect of coverage on mortality by almost 60% and increases the range of overlap in the estimated confidence intervals (column (3)).[38]

36. The SSDI Accelerated Benefits demonstration project (Michalopoulos et al. 2011; Weathers and Stegman 2012)—the only other randomized study of health insurance access on mortality of which we are aware—also failed to detect a beneficial effect of coverage on mortality. Like the Oregon study, its population, which was limited to 18-54-year-olds, was substantially younger than our mortality analysis sample. In addition, Weathers and Stegman (2012) report that the estimated effect of coverage on mortality for the SSDI study population may have been confounded by baseline differences in the prevalence of early-stage cancers among treatment and control group members.

37. To compare results across studies, we focus on the 20% of the Oregon replication data that includes survey data on individuals' months of coverage.

38. A different possibility is that the discrepancy is due to our observed mortality reduction being driven primarily by Exchange (rather than Medicaid) coverage, which the Oregon study intervention did not induce. However, this hypothesis is not consistent with Sommers, Baicker, and Epstein (2012), Miller et al. (2019), or Borgschulte and Vogler (2020), which all find that expanded Medicaid access did reduce mortality.

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Turning to the nonexperimental literature, our ability to compare our estimates to the results from the quasi-experimental studies referenced above is limited because most of these studies report mortality and coverage effects only over a four- or five-year time horizon. If the relationship between coverage and mortality is concave (as discussed in the prior subsection), the average per-month effect of coverage estimated in such studies is not comparable to ours. Luckily, although Miller et al. (2019) primarily focus on four-year mortality in their study of the effect of the ACA Medicaid expansions, they also report estimates for the mortality and coverage effects in the first two years following the expansion.[39] These estimates imply an effect of coverage on two-year mortality that is similar to the one we estimate (Online Appendix Table A.XXVII, column (4)).

Another useful benchmark from the prior quasi-experimental literature is evidence about the magnitude of the mortality reduction from coverage through specific channels. For example, Doyle (2005) and Card, Dobkin, and Maestas (2008) find that health insurance reduces short-term mortality following emergency department visits by 39% and 20%, respectively. A back-of-the-envelope calculation that incorporates these estimates in conjunction with our first-stage results suggests differences in emergency treatments due to coverage account for between one-third to one-half of our observed effect of the intervention on coverage, although differences across study populations and settings make direct comparisons difficult (see Online Appendix B.4).

Finally, although our results are qualitatively consistent with much of the prior evidence that health insurance coverage reduces adult mortality, an important difference from the previous literature is the type of variation in coverage that we study. Whereas most prior studies expanded access to free coverage among those previously ineligible for it, our intervention did not alter the coverage our sample population could access. Rather, it induced individuals to enroll in coverage that was already available to them. In a standard adverse selection model, one might expect that such individuals are unlikely to benefit much from coverage—otherwise, they would have chosen to sign up even

39. Miller et al. (2019) measure uninsurance at a single point in time each year rather than on a monthly basis; to compare our estimate to their, we assume that each individual induced to obtain coverage because of the Medicaid expansion they study does so for each month in the year.

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absent the intervention. In contrast, our finding that the intervention reduced mortality for this group suggests that the behavioral frictions that reduce coverage take-up may be particularly concentrated among those individuals who would benefit from enrolling.

VI. Conclusion

We studied the effects of an outreach program that highlighted tax incentives to enroll in health insurance and that was directed at taxpayers who had previously incurred a penalty for lacking coverage. We interpret our results to support a number of hypotheses, with varying degrees of confidence. First, the randomized nature of our design provides strong evidence that the outreach intervention increased coverage and reduced short-term mortality among middle-aged adults. Second, we interpret our results to support the conclusion that the coverage induced by the intervention reduced mortality. Attributing the observed mortality reduction to the intervention-induced coverage requires ruling out other channels through which the intervention may have reduced mortality. Although other channels are certainly imaginable, they appear less likely than coverage to explain the intervention's effect on mortality.

Third, our results shed light on the magnitude of the causal relationship between coverage and mortality; however, this element of our findings is subject to a greater degree of uncertainty. One reason for this uncertainty is that the IV analysis requires two additional assumptions: monotonicity and an assumption concerning the particular margin of coverage through which the intervention affected mortality. Although these assumptions strike us as plausible, and we observe no direct evidence for their violation, their failure could bias the magnitude of our IV estimate upward in magnitude. Another source of uncertainty is statistical imprecision: our estimated confidence interval is consistent with both moderate and very large effects of coverage on mortality. As discussed, we view the effects in the lower-magnitude portion of this range as most plausible. Combining our findings with results from other research on this topic through meta-analyses, as proposed by Sutton and Abrams (2001), is one potential path for more precisely estimating the relationship between coverage and mortality.

Our results also speak to important policy questions surrounding the use of outreach strategies to increase health insurance coverage. Ex ante, one might predict that the individuals

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who choose to forgo coverage (absent outreach) would be those for whom the health benefits of coverage tend to be small, especially when the outreach concerns financial penalties for remaining uninsured. However, this is precisely the group that identifies our estimated effect of coverage on mortality. Our results therefore suggest that behavioral frictions like salience or inattention shape how tax incentives interact with adverse selection in health insurance markets; the behavioral frictions that reduce coverage take-up may be particularly concentrated among those who would benefit from enrolling. As a result, outreach efforts of the type we study may yield substantial health benefits.

An important limitation of our analysis is that we observe only two years of outcome data postintervention. As a result, our results speak only to the short-term effects of coverage on mortality. Longer-term mortality effects may be present as well. For example, coverage may induce treatment for chronic conditions that would otherwise hasten, but not immediately cause, mortality, such as liver or kidney disease or some forms of cancer (Sommers et al. 2017a). Similarly, additional years of outcome data would shed light on the longer-term survival prospects of those whose lives were extended because of the new coverage. Depending on the persistence of the coverage effect, additional years of outcome data could also shed light on the steady-state effects of the new coverage on mortality, as well as on the curvature of the relationship. Although we hope to study these questions in future work as additional data become available, we note that the effective repeal of the individual mandate in 2019 may limit the effect of the intervention on coverage to 2017 and 2018.

Finally, although mortality is an important input into welfare, we lack data on many of the other factors that would enter a careful cost-benefit analysis of outreach, such as financial well-being and health outcomes other than mortality. Along the same lines, because we lack data on health expenditures, we are unable to investigate how the intervention shaped adverse selection in health insurance markets. In future research, we hope to link the pilot study to data that would permit consideration of these questions.

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Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

Data Availability

Code replicating the tables and figures in this article can be found in Goldin, Lurie, and McCubbin (2020), in the Harvard Dataverse doi: 10.7910/DVN/SNKACY.

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