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Response to "Mortality and Science: A Comment on Two Articles on the Effects of Health Insurance on Mortality"

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Link to Abstract

In Sarah Miller, Norman Johnson, and Laura Wherry (2021), we used federal administrative mortality data linked to the American Community Survey (ACS) to examine the health effects of the Affordable Care Act (ACA) Medicaid expansions for near-elderly, low-income adults who gained Medicaid eligibility. We find a significant reduction in mortality following the ACA expansions for this targeted group of adults in Medicaid expansion states, as compared to non-expansion states. We used administrative Medicaid enrollment data to document the corresponding change in Medicaid coverage, as well as cause-of-death data to determine that the effect was driven by a reduction in the types of deaths affected by access to medical care.

Robert Kaestner (2021) questions the plausibility of our findings in a comment that addresses our study and another paper by Jacob Goldin, Ithai Lurie, and Janet McCubbin (2021), which also documents the mortality effects of health insurance coverage. Kaestner's main critique is that our study lacks sufficient statistical power to estimate the effect of the Medicaid expansions on mortality and to detect non-parallel pre-trends across the expansion and non-expansion states. In this response, we show that Kaestner's calculations included several implementa-

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tion errors that led him to incorrectly estimate power for our study. Once corrected, we find greater statistical power than what is reported in Kaestner (2021). Furthermore, we discuss how the assumptions used by Kaestner to interpret his calculations are implausible. Other comments brought up by Kaestner are already addressed in the published version of our paper and its appendix, and we direct interested readers there.

Statistical power

The primary criticism made by Kaestner (2021) is that our study lacks the statistical power necessary to detect an effect of health insurance on mortality. To make this argument, Kaestner conducts a power-calculation exercise using the public-use version of the ACS. However, as we demonstrate in this response, the analysis in Kaestner (2021) hinges critically on a series of implausible assumptions, as well as implementation errors that lead him to understate statistical power.

Incorrect power calculations

We begin by discussing two key implementation errors in the simulation exercise conducted by Kaestner (2021) that led him to inaccurately represent the analysis undertaken in our study. First, the data set used by Kaestner has markedly fewer observations than that used in Miller et al. (2021), leading to an inaccurate assessment of statistical power. Specifically, Kaestner uses the public-use version of the ACS, whereas the analysis in Miller et al. (2021) is based on the restricted-use version of the ACS that contains a sample approximately 33 percent larger. Second, in contrast to our paper, the analysis undertaken by Kaestner (2021) does not use ACS survey weights. To correct these errors, we first resample the public-use ACS with replacement to achieve a more accurate sample size. We then apply annual mortality rates to the weighted sample and estimate mortality changes in specifications that use survey weights.

As shown in Table 1, when we make these corrections, we observe a strikingly different set of estimates of statistical power than what is reported in Kaestner (2021).[3] The level of statistical power for effect sizes of −0.0006,

3. Replication code for our estimates is posted on the journal site (**link**). All replication and corrections to the Kaestner (2021) power calculations are based on the Stata data and do files provided to us by Kaestner. Since his code does not specify an initial value for the random-number seed used in his simulations, we are unable to exactly match the estimates reported in his comment. Corrected estimates reported in Table 1 increase sample size by 33 percent, apply survey weights, and remove individuals from the panel following assigned death. This last correction has only a minor effect on the statistical power estimated.

−0.0007, and −0.0008 all meet or exceed the threshold of 80 percent, the conventional measure of sufficient power. This indicates detectable effect sizes that represent as little as 4 to 6 percent of the sample mean, a 1.4 percent mortality rate. As we discuss in more detail below, these assumed effect sizes are well within the range of plausible estimates when considering both the share of the population gaining exposure to Medicaid and their higher-than-average baseline mortality rate.

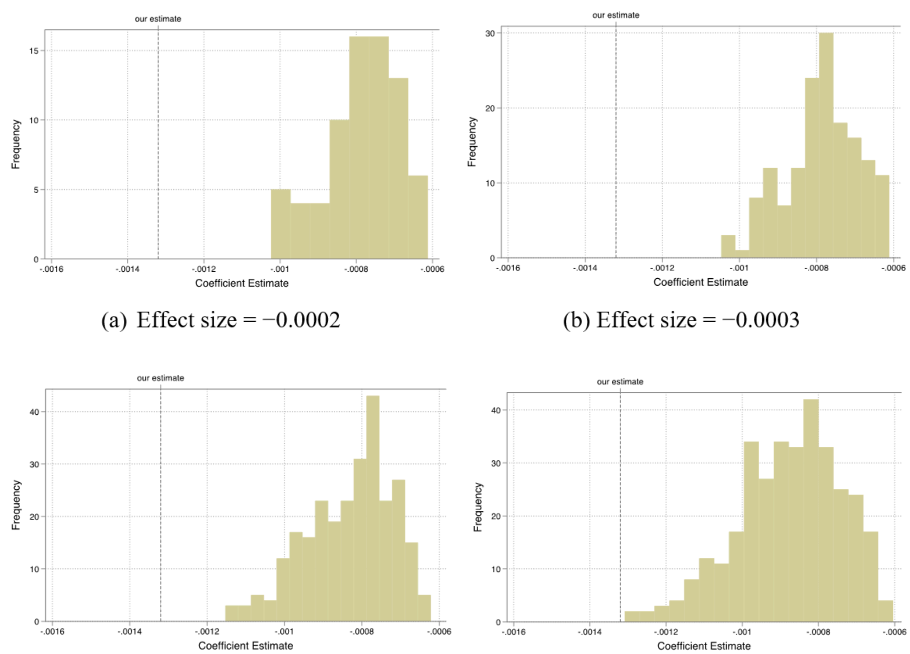
TABLE 1. Analysis of statistical power in Miller et al. (2021)

Assumed effect of Medicaid expansion on mortality	Share of estimates statistically significant (statistical power)		
	Kaestner (2021)	Our replication	Corrected estimates
	(1)	(2)	(3)
−0.0002	14%	13%	15%
−0.0003	23%	27%	31%
−0.0004	37%	32%	54%
−0.0005	47%	44%	66%
−0.0006	64%	63%	80%
−0.0007	74%	73%	91%
−0.0008	85%	82%	96%

Notes: Analyses completed with Stata data and do files provided by Kaestner. Corrected estimates increase sample size by 33 percent, apply ACS survey weights, and remove individuals from the panel following assigned death.

Greater examination of the results from these power calculations further indicates that it is highly unlikely that our estimated mortality effect is an overestimate resulting from a low-powered analysis, as Kaestner argues. If that were the case, our estimate would fall within the range of simulated estimates derived from the imposition of a small mortality effect. However, the actual estimated effect size in Miller et al. (2021) is −0.00132, which falls outside the range of estimates in any of the lower-powered analyses. To demonstrate this, Figure 1 shows the distribution of significant estimates for each of the assumed effect sizes where we have less than 80 percent power, as reported in Table 1. Not only is the estimate from Miller et al. (2021) larger in size than any of the significant estimates in these simulations, but it also falls well below the range of estimates for the smallest imposed effect sizes. This suggests that Kaestner's claim that our estimate is a spurious finding resulting from a lack of statistical power is unlikely to be true.

Figure 1. Distribution of significant coefficient estimates by assumed effect size



(a) Effect size = −0.0002

(b) Effect size = −0.0003

(c) Effect size = −0.0004

(d) Effect size = −0.0005

Notes: Each plot shows the distribution of statistically significant estimates from the corrected version of Kaestner's simulations for assumed effect sizes with less than 80 percent statistical power as shown in Table 1.

Implausible interpretation

Given the reduced-form nature of our estimates, assumptions are necessary to provide information on the implied treatment effect of Medicaid coverage for the individuals who enrolled. We approach this exercise with great care in our paper and devote an entire subsection to a transparent discussion of this type of interpretation. In the discussion below, we highlight how the narrow and implausible assumptions used by Kaestner in his comment result in a misleading interpretation of the effects presented in Miller et al. (2021). The points we make here are drawn directly from Section VIII.A of Miller et al. (2021, 1817–1820) and we would point Kaestner and other interested readers to additional information there.

First, Kaestner (2021) uses the estimated change in net self-reported health insurance coverage to scale and interpret the imposed mortality effects, rather than the estimated change in actual treatment (i.e., the receipt of Medicaid). As we discuss in our paper, there are both theoretical and practical reasons to scale by the change in Medicaid coverage, which was 2.9 times larger than the estimated change in net insurance coverage. Notably, scaling by the change in net insurance coverage ignores any benefits derived from Medicaid coverage among enrollees who would otherwise have purchased private insurance coverage. It also implicitly assumes that Medicaid and private coverage affect mortality in an identical manner.

This is a strong assumption given that Medicaid and private coverage are distinct in many ways and likely to affect health, finances, and the use of medical care differently. For example, low-income adults enrolled in coverage through the ACA exchanges face significantly higher expenses to use medical care and use fewer health care services when compared to observably similar Medicaid enrollees (Blavin et al. 2020). Exchange plan enrollees often face a more restrictive set of hospitals at which they can seek care since a large percent of exchange plans are considered to have a "narrow network" (Dafny et al. 2017); network breadth may be particularly relevant for hospital care given that high quality hospitals appear to causally reduce mortality, at least for acute cardiovascular events (Doyle et al. 2015). Finally, new research casts further doubt on the assumption that Medicaid and private plans are equivalent by demonstrating that the design of insurance plans and cost-sharing causally affects mortality, and that these effects are large (Abaluck et al. 2021; Chandra et al. 2021). Column (1) of Table 2 displays the implied effects of annual Medicaid enrollment, rather than insurance coverage, on mortality.

TABLE 2. Interpretation of assumed mortality effects

Assumed effect of Medicaid expansion on mortality	Implied effects for newly covered			% relative to full sample			% relative to compliers		
	Medicaid (CMS)	Any insurance (ACS)	Any insurance (NHIS)	Medicaid (CMS)	Any insurance (ACS)	Any insurance (NHIS)	Medicaid (CMS)	Any insurance (ACS)	Any insurance (NHIS)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
−0.0002	−0.0016	−0.0045	−0.0034	−10%	−28%	−21%	−5%	−15%	−12%
−0.0003	−0.0023	−0.0068	−0.0052	−14%	−42%	−32%	−8%	−23%	−18%
−0.0004	−0.0031	−0.0091	−0.0069	−19%	−56%	−42%	−11%	−31%	−23%
−0.0005	−0.0039	−0.0114	−0.0086	−24%	−70%	−53%	−13%	−39%	−29%
−0.0006	−0.0047	−0.0136	−0.0103	−29%	−84%	−63%	−16%	−46%	−35%
−0.0007	−0.0055	−0.0159	−0.0121	−34%	−98%	−74%	−19%	−54%	−41%
−0.0008	−0.0063	−0.0182	−0.0138	−38%	−112%	−85%	−21%	−62%	−47%

Notes: Estimates in column (1) are equal to the assumed effect on mortality divided by 0.128, which is the estimated change in annual Medicaid enrollment. Estimates in columns (2) and (3) divide the assumed effect on mortality by 0.044 and 0.058, respectively, which are the estimated changes in any insurance coverage from the ACS and NHIS. Estimates in columns (4)–(6) divide the estimates in columns (1)–(3) by 0.0163, which is the counterfactual mortality rate for the full sample. Estimates in columns (7)–(9) divide the estimates in columns (1)–(3) by 0.0295, which is the counterfactual mortality rate for the complier sample. See Miller et al. (2021) for further details on the first stage and counterfactual mortality estimates used here.

Practically, as discussed in our paper and in contrast to our analysis of changes in Medicaid enrollment, we do not have access to administrative data on net insurance coverage for our sample. Therefore, we are unable to estimate actual changes in insurance coverage and, instead, rely on an analysis of self-reported, point-in-time coverage from repeated cross-sections of ACS data for individuals with similar characteristics. This is less than ideal given the potential for measurement error in self-reported health insurance coverage (Lurie and Pearce 2021). We alternatively use self-reported insurance coverage information from the National Health Interview Survey (NHIS), which is considered to have the most valid insurance coverage information among the federal surveys (Lynch et al. 2011), since the NHIS is conducted in person (rather than mail-in or online) and includes a verification question regarding insurance status. When using this measure of insurance coverage, we estimate a 30 percent larger change in insurance coverage; column (3) of Table 2 displays the implied effects of insurance coverage using this estimate from the NHIS.

In addition, scaling by these point-in-time measures of insurance fails to capture impacts of health insurance coverage that accumulate over time, or that extend beyond the period of coverage. These cumulative effects could occur if, for example, health insurance coverage last year results in better health today, regardless of current insurance status.[4] Such extended effects of health insurance on health are documented in our existing joint work with Dr. Kaestner (see Wherry et al. 2017), but would not be accounted for in the limited interpretation of the estimates provided by Kaestner (2021). In Miller et al. (2021) we demonstrate that not only did respondents in our sample experience higher rates of point-in-time insurance and Medicaid coverage, but they also accumulated more years of exposure over the sample period; see our discussion in Section V.A (Miller et al. 2021, 1800–1803).

Second, Kaestner (2021) presents assumed mortality effects relative to the counterfactual mortality rate for the *entire* sample, not those who took up expanded Medicaid coverage. This calculation assumes that there is no adverse selection into the Medicaid program—that individuals who are induced to enroll in Medicaid (i.e., the "compliers") have the same baseline health as those who gain eligibility but opt not to enroll. However, ample empirical evidence suggests that it is unlikely that individuals choose to enroll in Medicaid at random (see, e.g., Garthwaite et al. 2019; Kenney et al. 2012; Marton and Yelowitz 2015). Instead, it is far more plausible that those who stand to benefit the most from Medicaid coverage (e.g., those with serious health needs) are more likely to enroll in the program. In our

4. Kaestner (2021, 203) mentions this possibility himself: "One possibility…is that cumulative health insurance coverage and the care it implies has an increasingly beneficial effect."

data, we observe an annual mortality rate of 2.0 percentage points for individuals who enroll in Medicaid in expansion states in the post-ACA period, far higher than the average rate observed in the already disadvantaged population we study. Using this observed mortality rate, we estimate a counterfactual mortality rate for the compliers that also considers the mortality reduction benefits of Medicaid conferred to this group. Use of this alternative counterfactual rate results in much smaller proportional treatment effects (see columns (7)–(9) of Table 2).

As discussed in the paper, we consider this complier counterfactual mortality rate to be an upper-bound estimate of the true counterfactual since we are unable to identify the eligibility category in our administrative Medicaid data. Some of these individuals likely would have qualified for coverage based on a disability even if there were no eligibility expansions. However, bearing this in mind, we expect that the true counterfactual mortality rate falls somewhere between the rates estimated for the full and Medicaid-enrolled samples in our data. Therefore, we expect that the true proportional effect falls somewhere between the estimates presented in columns (4)–(6) and those in columns (7)–(9) of Table 2.

Bearing these factors in mind, we provide a more realistic interpretation to the corrected power calculations reported in Table 1. As shown there, we are powered to detect, at a minimum, a mortality decrease of −0.0006 in our analysis in Miller et al. (2021). Depending on the counterfactual mortality rate used, this suggests between a 29 percent (column 9) and 63 percent mortality (column 6) decrease resulting from new insurance coverage, as measured by the NHIS, or between a 16 percent (column 7) to 29 percent (column 4) mortality decrease resulting from new Medicaid enrollment, as measured using administrative data. All these estimates fall well below the 100 percent reduction in mortality claimed by Kaestner.

Power to detect differential pre-trends

Kaestner (2021) states that our evidence regarding parallel pre-trends is unpersuasive due to lack of statistical power. However, the power calculations we provide in the paper, based on the approach developed by Jonathan Roth (2019), show that we are adequately powered to reject differential trends in mortality across the expansion and non-expansion states. We further demonstrate that if we account for the potential bias under a "worst case" scenario, such that the largest non-detectable pre-trend is present, the true effect would still represent a meaningful reduction in mortality. As we state in the text:

> Although we do not find any evidence of differential pretrends in the event

> study, we explore how well powered this test is in our context. To do this, we first determine the size of a linear pretrend we are powered to detect following the procedure described in Roth (2019). Our analysis suggests that we could detect with 80% power a fairly small negative linear trend of a magnitude of 0.03235 percentage points or greater (in absolute terms) in our event study model (i.e., a pretrend of such size is likely to generate at least one statistically significant preperiod event study coefficient). If a trend of a size of up to −0.03235 percentage points is indeed present (although not detectable to us), we calculate that it would generate, by year 3 following the expansion, a bias of at most −0.08873 percentage points.[5] Our actual estimate in year 3 is −0.2082 percentage points, and substantially larger (2.3x) than this potential bias. (Miller et al. 2021, 1808–1810)

Kaestner (2021) also makes a claim that is tangentially related to the results in Miller et al. (2021): that employment outcomes evolved differently across expansion and non-expansion states prior to the ACA. It is implied by Kaestner that these employment trends may (through an unspecified mechanism) result in differential trends in mortality rates. However, we find no evidence of differential trends in mortality, which, as discussed above, we are powered to detect. In addition, we undertake several additional analyses in the paper that (1) allow for differential pre-trends in mortality among counties with differing economic and demographic characteristics, (2) allow for different groups of states to have differential linear pre-trends, and (3) test the sensitivity of our estimates to controls for local economic conditions. None of these alternative specifications meaningfully changes our results; see the detailed discussion in Section VI.C of the paper (Miller et al. 2021, 1810–1812).

Conclusion

Kaestner (2021, 209) dismisses the results of Miller et al. (2021) as “implausible” and claims that, as a result, there is nothing to be learned from this paper. On the contrary, the estimates provided in Miller et al. (2021) are consistent with those documented in other settings, both experimental and quasi-experimental. Furthermore, once the implementation errors in Kaestner’s code are corrected, his own power calculations demonstrate that we have more than adequate statistical power to support the analysis provided in our paper. Other critiques highlighted in Kaestner (2021) are either already addressed in the paper

5. We calculate the bias following the formula presented in Roth (2019), which takes into account the additional bias introduced by passing a pre-test.

or only tenuously related to the main results of Miller et al. (2021). In conclusion, the discussion in Kaestner (2021) does not lead us to alter our interpretation of the results presented in Miller et al. (2021): expanding Medicaid coverage to low-income, near elderly adults reduces their mortality risk.

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