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Political Death Creep: Revisited Using Hausman-Taylor

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Abstract

In an analysis of Covid-19 death recording in 2020, Kunce (2020a) examined whether the perceived political ideology of a state in the U.S. impacted Covid-19 assigned deaths. The idea being that the political 'attitudes' of those responsible for completing and certifying death certificates influenced whether Covid-19 appeared as a cause of death under the Centers for Disease Control and Prevention's new liberal guidance. States that lean more democrat in ideology were found to assign significantly more Covid-19 related deaths than the average state − coined a blue-state political death creep. This paper extends the analysis using state-level panel data from 2020-2022. Results from a Hausman-Taylor instrumental variable model bolsters the conclusions reached by Kunce (2020a) with robust specification.

JEL classification numbers: C12, C13, C23.
Keywords: Covid-19 deaths, Panel data, Hausman-Taylor.

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1. Introduction

From the weeks ended January 11, 2020 through March 21, 2020, the National Center for Health Statistics (NCHS)[2] in the U.S. recorded 717 Covid-19 related deaths (NCHS 2022a). The following week ended March 28, 2020, 3,226 Covid-19 related deaths were recorded − four-and-one-half times more deaths in one week than the prior eleven weeks combined. During the ensuing week ended April 4, 2020, 10,141 Covid-19 deaths were assigned. Coincidentally, on March 24, 2020 the NCHS, National Vital Statistics System (NVSS) released formal guidance on how to "accurately" designate Covid-19 on U.S. death certificates (NVSS 2020a). A new International Certification of Death (ICD) code, U07.1, was introduced for "immediate" use on death certificates. The alert went on to instruct, "Covid-19 should be reported on the death certificate for all decedents where the disease caused[3] or is *assumed* to have caused or contributed to death" and "the rules for coding and selection of the underlying cause of death are expected to result in Covid-19 being the underlying cause more often than not".[4] This new guidance usurped prior death certification precedent in the U.S. (see CDC 2003b, CDC 2003c, NVSS 2020b, CSTE 2020). Government and public health officials insisted that this new, more liberal, guidance would help provide a wide-ranging surveillance of how lethal the new disease was and is. Others were fearful of a more baleful, political purpose intending to exploit exaggerated death counts.

In an analysis of the more menacing view of Covid-19 death recording, Kunce (2020a) examined whether the perceived political ideology of a state in the U.S. impacted Covid-19 assigned deaths. The idea being that the political 'attitudes' of those responsible for completing and certifying death certificates influenced whether Covid-19 appeared as a cause of death using the new liberal guidance. Political attitudes were captured by a states' 2016 partisan presidential vote counts.[5] Results from two regression specifications, based on 2020 data, showed that states tending to vote more democrat assigned significantly more COVID-19 deaths, compared to the average state, after controlling for population density and share of aged population. Kunce (2020a) concluded that results suggested that the CDC/NCHS directed liberal death reporting guidance for COVID-19 fostered an environment for a blue-state political death creep. To date, Covid-19 death assignment continues. On October 13, 2022, the Biden administration formally extended the public-health emergency for the U.S. into January of 2023 (Seitz 2022). The current plan is to keep the emergency operational to the end of April

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[2] Part of the Centers for Disease Control and Prevention (CDC).

[3] Confirmation of the disease via testing is not without error. Covid-19 tests wildly vary in their sensitivity (false negatives) and specificity (false positives). Moreover, no qualified virus isolate, to use as a testing standard, currently exists (CDC 2022a; Kunce 2020b).

[4] The latter guidance, in effect, subordinates chronic comorbidities on COVID-19 related death certificates.

[5] Specifically the Hillary Clinton vote percentage minus the Donald Trump vote percentage by state. As the voting share difference increases, the state residents are presumed to be more Democrat (blue) in ideology.

2023 (Adcox 2022). First enacted January 31, 2020, the Covid public-health emergency has been renewed every 90 days since. As of the week ended June 4, 2022 through November 11, 2022, an average of roughly 2,500 assigned Covid-19 related deaths were being recorded per week in the U.S. (NCHS 2022a).

The Kunce (2020a) analysis has been criticized, mainly, because it was based on roughly one year of mortality data. Now that the Covid-19 state of emergency in the U.S. is continuing through a third year, more informative panel data is available. An important benefit from pooling the now 3-year cross-state data is the ability to control for state- and time-specific effects, possibly unobservable, which may be correlated with other covariates in the specification. Analysis of cross-section or time-series data alone are deficient in identifying or controlling for such confounding effects. The purpose of this examination is to extend Kunce (2020a) using a 3-year panel across all 50 U.S. states and the District of Columbia. Rather than using state-level national election data for the ''attitudes' proxy, we substitute with the partisan composition of each jurisdiction's legislature. Within-state composition of the local legislative body better represents the diverse political ideology stemming from 'all-corners' of a state's borders. Moreover, we include a new covariate capturing the non-white population share of a state. Analyses in 2020 suggested that the non-white population in the U.S. disproportionately suffered death outcomes from Covid-19 (Keating et al. 2020). The balance of this examination is divided into four sections. Section 2 describes the data, provides sources, presents descriptive statistics and variable analysis. Section 3 presents the empirical model and discusses the complex econometric issues. Section 4 interprets the empirical results with conclusions and future research directions drawn in section 5.

2. Data

Table 1 presents the data, sources and relevant descriptive statistics. The dependent variable, Covid%, is calculated as the percentage share of all Covid-19 related deaths (certificates coded with ICD-10 U07.1) to the all-cause death total for each state by year.

Table 1: Data descriptions, sources, descriptive statistics

Covid%. Share of Covid-19 related deaths to total deaths by state of occurrence and year, in percent. Covid-19 related deaths are denoted as presumed or confirmed with death certificates coded ICD-10 U07.1. National Center for Health Statistics, National Vital Statistics System, 2020-2022. 2022 deaths are provisional, last accessed on November 9, 2022. 2020 Mean 10.61, STD 3.79; 2021 Mean 12.27, STD 2.80; 2022 Mean 8.18, STD 1.38; Pooled Mean 10.35, STD 3.29. Covid Deaths Mean 6,929, STD 8,584; Total Deaths Mean 61,036, STD 63,517.
Wedge%. Based on state-specific, midyear legislative partisan composition. Calculated as Democrat held seats minus Republican seats as a share of total seats, in percent. National Conference of State Legislatures, 2020-2022. Mean -7.26, STD 41.77.
PopDensity. Population of a state per square mile of bordered land area. United States Census Bureau, 2020-2022. Mean 420, STD 1,540.
65Plus%. Percent of a state's population aged 65 and above. United States Census Bureau, 2020-2022. Mean 17.55, STD 1.99.
NonWhite%. Percent of a state's population categorized as non-white. United States Census Bureau, 2020-2022. Mean 25.67, STD 13.54.

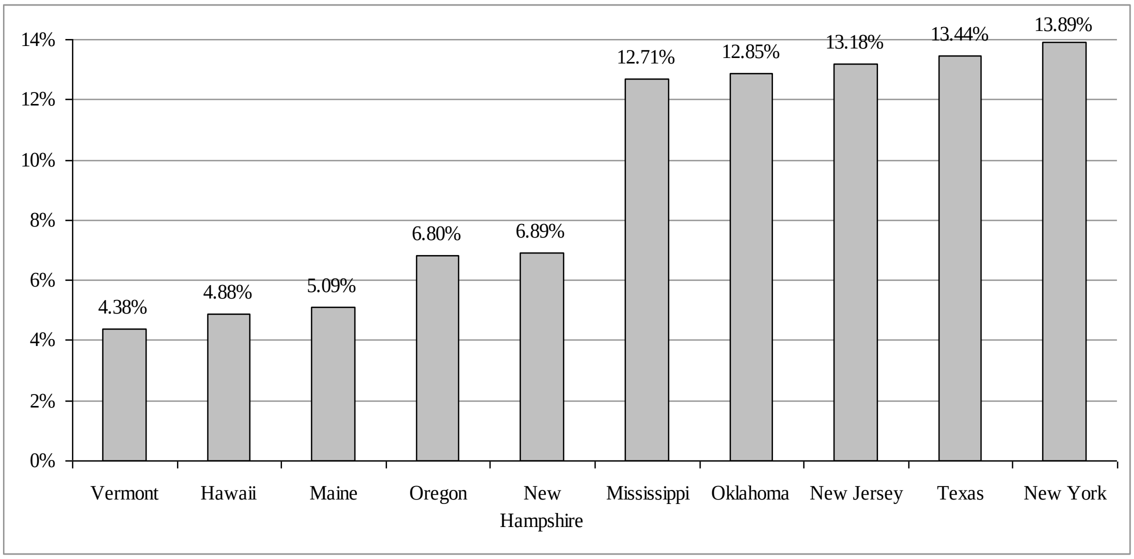

Figure 1: **Five lowest and highest state-mean Covid death percentages**

Figure 1 shows the 5 states with the lowest and the 5 states with the highest state-mean Covid-19 death percentages. Table 1 shows that just over 10 percent (pooled mean) of all deaths in the U.S., for roughly the last 3 years, were assigned as Covid-19 related. Interestingly, 2021 had the highest Covid-19 mean-death percentage, 12.27%. Starting on the right-hand-side (RHS), the Wedge% variable is derived as follows,

$$\frac{\text{Democrats (State House and Senate)} - \text{Republicans (State House and Senate)}}{\text{Total Seats (State House and Senate)}} \otimes 100 \qquad (1)$$

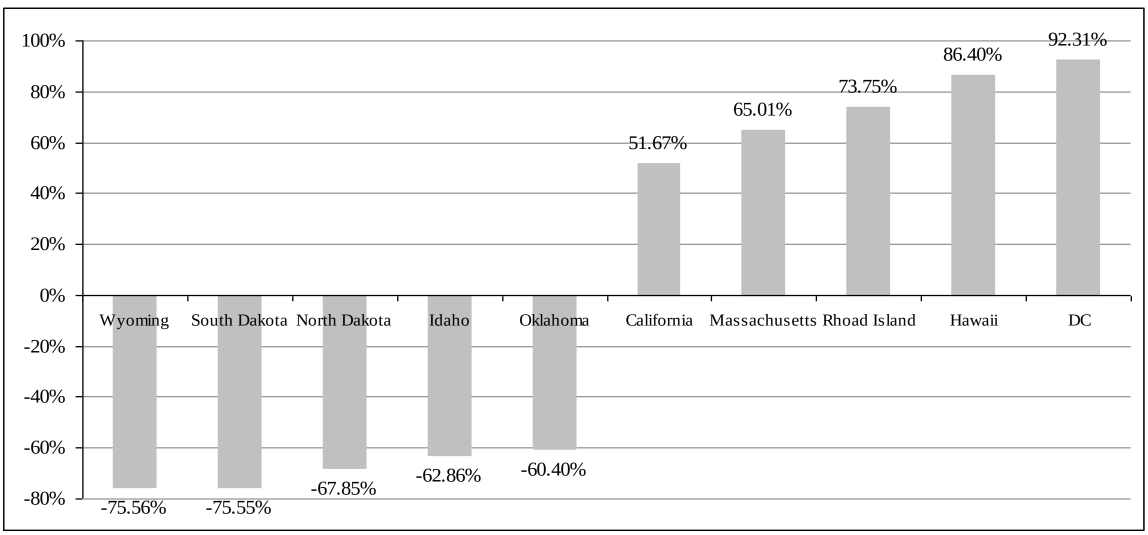

Figure 2: Five lowest and highest state-mean partisan wedge percentages

The partisan makeup of each state's legislature reflects the midyear (around June) seats held. A larger Wedge% reflects a more Democrat leaning 'attitude' of a particular state. Figure 2 shows the top five more republican leaning states along with the top 5 more democrat leaning states. Population density (PopDensity) is a control for how the disease is said to behave, congested jurisdictions appear to suffer infection and death the most. Moreover, the aged (65Plus%) face the highest mortality risk with roughly 75% of the total COVID-19 assigned deaths occurring in the population of those 65 and above (NCHS 2022b). Lastly, NonWhite% reflects the share of a state's population classified as non-white − a group with one of the highest risks of death from Covid-19 in 2020. For example, Blacks in the U.S. were 37 percent more likely to die from Covid-19 than whites based on 2020 data (Keating et al 2020).
In addition to the variable particulars denoted in Table 1, Table 2 depicts the variable correlations and variance inflation factors (VIF). The diagnostic tools shown in Table 2 are standard in detecting multicollinearity. While multicollinearity problems are certainly a matter of degree, the risk of deleterious issues herein appears small.

Table 2: Correlation matrix and variance inflation factors

	Wedge%	PopDensity	65Plus%	VIF
Wedge%	1.00	-	-	1.58
PopDensity	0.41	1.00	-	1.28
65Plus%	0.13	-0.07	1.00	1.15
NonWhite%	0.49	0.38	-0.21	1.53

3. Model

Consider the following two-way time-series and cross-section model,

$$Y_{it} = X_{it}\beta + Z_i\gamma + \varepsilon_{it} \qquad (i = 1, \ldots, N;\ t = 1, \ldots, T;\ n = NT), \qquad (2)$$

where Y_{it} is the dependent variable, X_{it} are observable variables that vary across states i and over years t, Z_i are observable time-invariant variables, β and γ are k and f vectors of estimated coefficients and ε_{it} denotes the overall error term.[6] The error term is comprised of three components,

$$\varepsilon_{it} = \mu_i + \lambda_t + \nu_{it}, \qquad (3)$$

where μ_i denotes the unobservable state-specific effects, λ_t is the latent year-specific effects and ν_{it} is the remainder stochastic disturbance. The component μ_i is time-invariant and will account for state-specific effects not included in the RHS. The

[6] Many presentations of this familiar model may include a scalar constant term, α.

year-specific effect, λ_t, is state-invariant and will account for any time-specific effects not included in the regression. The remainder disturbance v_{it} varies with groups and time and is assumed orthogonal to X, Z, μ and λ with a mean of zero and a constant variance σ_v^2.

Generally, two specifications of equation (2) are considered and differ based on their treatment of the specific effects. First, 'fixed effects' (FE) treats μ_i and λ_t as fixed but unknown constants differing across states and time. This specification is easily estimated by including dummy variables in the RHS (Least Squares Dummy Variable (LSDV) estimator). However, as in our case (N = 51, T = 3), LSDV suffers from the loss of precious degrees of freedom. Alternatively, estimates can be obtained by transforming the data into deviations from respective state-means ('within' estimator) and by including T - 1 year dummies. The two fixed effects estimation methods described reveal two crucial defects: (i) time-invariant variables are eliminated so γ cannot be estimated, and (ii) the estimator is not fully efficient because, in certain cases, it ignores variation across states and/or time.

Second, 'random effects' (RE) assumes that the μ_i are random variables, distributed independently across groups with variance σ_μ^2. Because T is so short, we will include year dummies for λ_t resulting in a two-way model. Estimates of this specification are based on transformations of the data into deviations from weighted respective state-means where the weights are based on, generally, the estimated variances of the components in equation (3) and T (Feasible General Least Squares (FGLS) estimator). Specifically, the weight on the state-means takes the form,

$$\hat{\theta} = 1 - \frac{\hat{\sigma}_v}{\sqrt{T\hat{\sigma}_\mu^2 + \hat{\sigma}_v^2}} . \tag{4}$$

Note, if $\hat{\theta} = 1$, random effects is the 'within' fixed effects estimator. Unbiased robust estimates of the variance components are best obtained from pooled ordinary least squares (OLS) and LSDV estimators. The potential correlation of the specific effects with the variables in X_{it} and Z_i is a defect of the random effects construct. If these correlations are present, random effects estimation yields biased and inconsistent estimates of β and γ. Conversely, by transforming the data into deviations from the simple state-means the fixed effects estimator is not impacted by this lack of orthogonality.

Hausman (1978) and Kang (1985) outlined specification tests of the null hypotheses of orthogonality between the effects and the regressors. By failing to reject the null, both fixed effects and random effects are unbiased and consistent, but fixed effects is less efficient. When the null is rejected, fixed effects is unbiased and consistent but random effects is not. The random effects specification requires exogeneity of all regressors and the components in equation (3). Conversely, the fixed effects model allows for endogeneity of all the regressors and the specific effects, but ignores observable variables Z_i. In order to avoid this all or nothing choice of exogeneity and accommodate the estimation of γ, Hausman and Taylor (1981) (HT) propose a third specification for estimating equation (2) where the RHS is split into two main categories of variables, those assumed uncorrelated (exogenous) with the effects and v_{it}, and those correlated (endogenous) with the effects, but not v_{it}. Table 3 shows the four possible sets of observable variables for equation (2).

Table 3: Hausman-Taylor variable sets

	Exogenous	**Endogenous**
Time varying	X_1 is n x k_1	X_2 is n x k_2
Time invariant	Z_1 is n x f_1	Z_2 is n x f_2

The exogenous category identified serves two functions, (i) using state-mean deviations, unbiased estimates of the respective elements of β are produced, and (ii) the exogenous set and state-means provide valid instruments for the unbiased and efficient estimation of β and γ. An advantage of panel data is the formulation of instruments from *within* the model construct. The order condition for identification requires that k_1 (the number of regressors in X_1) is greater than or equal to f_2 (the number of regressors in Z_2). When $k_1 > f_2$, the model is over-identified and HT is more efficient than 'within' fixed effects.

The complex HT estimator requires prior knowledge that certain RHS variables in equation (2) are uncorrelated with the effects. Following the pretest procedure suggested by Kunce (2021), we sort the regressors into the categories presented in Table 3. The variable pretest proposed by Kunce (2021) builds on the pretest estimator suggested by Baltagi et al (2003) by providing the necessary foundation for regressor identification. As a first step, estimate equation (2) with both fixed ('within') and random effects (FGLS) specifications and subsequently construct the chi-squared distributed Hausman test statistic. This initial statistic becomes the base of comparison. As a side note, if the Hausman null hypothesis of orthogonality is not rejected, FGLS is unbiased, efficient and commonly the correct specification. If the null is rejected (as it is in our case, $\chi^2_4 = 19.77$), identifying the RHS variables that contribute to the size of the Hausman statistic estimated from equation (2) is of primary concern. Second, estimate succeeding Hausman statistics from re-specified models by dropping one sequential regressor each iteration. Table 4 contains these results.

Table 4: Correlation contribution tests*

	χ^2_3
Wedge%	12.08
PopDensity	19.70
65Plus%	19.49
NonWhite%	11.76

*All RHS variables χ^2_4 = 19.77, base of comparison.

Row one of Table 4 shows the Hausman statistic when the Wedge% variable is dropped from the RHS and PopDensity, 65Plus% and NonWhite% remain. Row two of Table 4 shows the Hausman statistic when the PopDensity variable is dropped from the RHS and Wedge%, 65Plus% and NonWhite% remain − and so on. Note that the Hausman test statistic reduces to 12.08 from 19.77 when the Wedge% variable is dropped from the specification. The Wedge% variable appears to be a significant 'correlation contributor' therefore pretests as likely endogenous. The same is true for the NonWhite% variable. Recall that the necessary condition for identification and efficient estimation of β and γ is that $k_1 > f_2$ and f_2 may be empty (Hausman and Taylor (1981) pp. 1385-1387). Thus, a natural sorting from Tables 3 and 4 follows,

X_1: PopDensity, 65Plus%,
X_2: Wedge%, NonWhite%,
Z_1: Constant,
Z_2: Empty.

The T -1 time dummies are included as exogenous variables (Wyhowski 1994).[7]

4. Inference and results

Careful testing depicted in Table 5 fails to reject the null hypothesis of homoscedastic disturbances indicated by three tests; cross-section likelihood ratio, period likelihood ratio and the pooled sample White statistic. The Pesaran CD test statistic of 0.71 fails to reject the null of weak cross-sectional dependence (Pesaran 2015). Two F-tests denoted confirm state and year heterogeneity and verify the importance of controlling for unobservable state and year effects (a two-way model). The Durbin-Watson statistic of 2.02 indicates that error serial correlation is not an issue (Bartels and Goodhew 1981).

[7] Thanks to Jeffery Wooldridge of Michigan State University for this suggestion. As he puts it, the passage of time is certainly exogenous.

Table 5: Testing inference

	Statistic	P-Value
Cross-section Heteroskedasticity LR Test	43.03	0.779
Period Heteroskedasicity LR Test	23.27	0.991
Pooled White Heteroskedasicity Test	3.83	0.429
Pesaran CD Test	0.71	0.478
State Effects vs Pooled OLS, F Test	2.01	0.000
Adding Year Effects, F Test	27.61	0.000
– –	–	–
Pooled Durbin-Watson	2.02	–

Table 6 shows the results of the three two-way error component models. The first column depicts the RE estimates which assume no correlation between the RHS and the specific effects. The second column of Table 6 presents the 'within' FE estimates. Comparing the FE and RE estimates using the Hausman test rejects the null hypothesis of orthogonality confirming that the RE model is misspecified. This initial Hausman test outcome justifies the use of the HT instrumental variable method. Given the variable pretest results from above, the last column of Table 6 shows the estimates using the HT routine in *LIMDEP 11*®. A Hausman test based on the difference between FE and the HT estimator fails to reject the null hypothesis of orthogonality. There are two degrees of freedom in this chi-squared test since there are two over-identifying conditions (the number of X_1 regressors minus the number of Z_2 regressors, see Baltagi et al. (2003). The variable pretest herein is shown to be valid, we cannot reject that the set of instruments are appropriate. Because of the over identification described above, the HT specification is more efficient and favored over FE.

Table 6: Two-way error components estimates

	RE	FE	HT
Wedge%	0.022***	0.130**	0.111**
PopDensity	-0.001	0.015	0.007
65Plus%	-0.171	0.739	0.557
NonWhite%	0.041	-1.028	-1.031
R-Squared	0.37[a]	0.68	0.42[a]
Hausman	19.77***	-	1.78

[a] No precise counterpart to OLS R^2 in these constructs.
Significance at the < 1% (***), < 5% (**) levels.

Interestingly, only the Wedge% variable and the joint state- and year-effects are statistically significant in each specification.[8] The HT Wedge% coefficient of 0.111 can be interpreted as follows. For every one standard deviation (41.77%) share increase favoring democrats (e.g., increasing the partisan difference from say 10% to 51.77%), for the average state, the Covid-19 death share, of the average state, increases by 4.64 (41.77*0.111). Recall the mean for the dependent variable is 10.35 percent (4.64/10.35 = 45%). Evaluated at the mean for total deaths (61,036), this represents 2,832 assigned Covid-19 deaths, for the average state. States that lean more democrat in their state legislatures assigned significantly more Covid-19 deaths, on average (mean deaths 6,929), controlling for population density, share of aged population and the percentage of the population that is non-white. This result bolsters the conclusions reached by Kunce (2020b) with a more robust specification.

5. Conclusion

Framing our results in the sharpest perspective − the only observable state demographic modeled that significantly influenced the assignment of Covid-19 deaths in the U.S. was political attitudes. In our perspective, states with more democrat leaning legislatures (blue) assigned, on average, more Covid-19 related deaths as a result of the new liberal death reporting guidelines. Deciphering a specific motivation is left to the reader. A reviewer on an earlier version of this paper proposed a reversed view. States that lean more republican (red) assigned fewer Covid-19 deaths, on average, in spite of the new death reporting guidelines. Either way, a states perceived political ideology was the sole observed significant influence. Teasing this out empirically, if possible, is a direction for future research.

In support of the view of a directive-fostered, blue-state political death creep, consider the following. First, if republican leaning states are undercounting, why are 3 of the most partisan (South Dakota, Oklahoma and Kentucky) in the top 10 of overall state-mean Covid-19 death shares?[9] Second, the Coronavirus Aid, Relief and Economic Security Act (CARES 2020), Title III, incentivized a Covid diagnosis with direct payments and/or subsidies to caregivers.[10] All states accepted this aid with open arms. Once a Covid diagnosis was on a patient's chart, the relaxed death reporting guidelines provided a path for Covid-19 to appear on a subsequent death certificate. While anecdotal, evidence of abuse of these financial directives was prolific in the media (e.g. Seaman 2020). Lastly, and close to home, on September 10, 2020, the Wyoming State Public Health Officer Dr. Alexia Harrist, in a virtual meeting with state lawmakers, had to defend Wyoming's Covid-19 related death

[8] An interpretive note, while the other covariates do change over time, the within-state variation is small. Little within variation in the data impacts the fixed effects estimates, lending more support for the Hausman-Taylor approach.

[9] See the Appendix for a, by state, list of the variables Covid% and Wedge%.

[10] For example, Section 3710 of the CARES Act provided a 20% increased overall payment for Covid-19 patients on Medicare.

statistics. As depicted in Figure 2, Wyoming is the most partisan republican state in the U.S. Many lawmakers questioned Dr. Harrist on what they thought were inflated state Covid-19 death counts. Harrist defensively assured the legislators, "every single death among a Wyoming resident that we have reported, a health care provider or the certifying provider or coroner has determined that Covid-19 was a cause or contributor to death" (Coulter 2020).

A more salient critique of the analysis herein is one regarding jurisdictional effects. The state level data may be too aggregated and perhaps masks important devolved elements. A more broad landscape, perhaps county level, could provide an expanded analysis with an even more robust inference.[11] However, collection of panel data for the 3,142 counties and county equivalents in the U.S. could be a daunting task, particularly for more recent 2020-2022 demographics. Again, a direction for future research. Lastly, in an altogether transparent world, a forensic examination of all 1,067,539 Covid-19 related death certificates (as of 11/9/2022) in the U.S. would be illuminating. Interestingly, "for over 5 percent (53,377) of these deaths, Covid-19 was the only cause mentioned on the death certificate. For deaths with conditions or causes in addition to Covid-19, on average, there were 4 additional conditions or causes per death" (NCHS 2022b). This admission by the NCHS sheds light on the overall influence and impact of the March 24, 2020 death reporting directives. Moreover, when coupled with the results herein, the admission raises the question, how many genuinely died *from* Covid-19?

[11] It has also been suggested to examine the death recording at the city level. A key drawback to this idea is − what cities? The sample section issues may be too grave to overcome.

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Appendix

Table A1: State-mean and standard deviations

	Covid%		Wedge%	
	Mean	**STD**	**Mean**	**STD**
Alabama	11.12	3.08	-47.86	1.89
Alaska	8.80	4.51	-20.56	2.55
Arizona	12.61	4.21	-5.56	1.11
Arkansas	10.81	2.43	-54.32	2.80
California	10.98	3.24	51.67	0.83
Colorado	10.38	2.40	21.00	1.00
Connecticut	11.23	4.61	26.02	4.48
DC	10.40	2.85	92.31	7.69
Delaware	10.05	1.10	27.42	4.27
Florida	10.59	3.57	-24.58	4.02
Georgia	11.31	3.58	-17.09	1.36
Hawaii	4.88	1.61	86.40	2.01
Idaho	10.59	4.10	-62.86	2.52
Illinois	10.37	2.37	28.81	0.56
Indiana	11.40	1.94	-45.11	2.14
Iowa	10.36	3.02	-19.11	5.05
Kansas	10.80	1.75	-37.58	1.60
Kentucky	11.45	3.06	-45.65	11.93
Louisiana	10.56	2.91	-33.56	1.06
Maine	5.09	2.88	15.05	4.69
Maryland	10.22	1.31	39.36	0.53
Mass	10.29	3.99	65.00	2.18
Michigan	10.56	2.38	-7.21	1.03
Minn	9.44	2.12	2.99	3.03
Mississippi	12.71	2.20	-28.93	1.85
Missouri	10.27	1.58	-39.76	1.92
Montana	10.63	3.56	-26.22	8.28
Nebraska	9.68	2.03	-32.65	2.04
Nevada	12.57	3.59	23.28	7.50
New Hampshire	6.89	1.56	1.49	14.23
New Jersey	13.18	5.19	24.72	6.99
New Mexico	12.43	2.66	28.57	0.89
New York	13.90	4.92	39.28	1.51
North Carolina	9.80	2.62	-13.14	2.23
North Dakota	12.28	4.53	-67.85	3.50
Ohio	11.44	2.16	-33.08	3.74
Oklahoma	12.85	2.70	-60.40	4.08

Oregon	6.80	2.66	23.33	1.11
Penn	11.25	2.20	-10.94	1.39
Rhoad Island	11.33	4.08	73.75	1.35
South Carolina	11.02	3.26	-28.82	2.56
South Dakota	11.74	4.70	-75.55	5.25
Tenn	10.91	2.98	-52.27	0.76
Texas	13.44	4.43	-13.08	0.84
Utah	8.40	2.62	-56.73	0.96
Vermont	4.38	1.88	34.81	2.10
Virginia	9.19	1.92	5.48	5.99
Washington	7.23	1.59	17.01	0.68
West Virginia	9.90	3.79	-39.05	18.97
Wisconsin	9.04	1.71	-22.98	1.16
Wyoming	10.48	4.97	-75.56	2.94